

FORM WITHOUT FORMALISM

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The systematisation of deductive inference can yield an account of the structure of sentences (propositions, thoughts) involved in such inference. In mainstream contemporary analytic philosophy, the idea of “logical form” is linked, explicitly or implicitly, with the idea of such a systematisation—paradigmatically, with translation into a notational system whose grammar is designed to track valid inference patterns. But this is not the only thing one might mean by “logical form”. An enquiry into the structures of thoughts can be motivated by considerations besides inferential behaviour. In this dissertation I adduce reasons for doubting that inferential systematisation can capture all of what we might want from the notion of logical form, and I sketch an alternative conception according to which the uncovering of the logical structure of discourse proceeds from no single principle (such as deductive inferential potential), but rather piecemeal, from region of discourse to region of discourse. In this sense the investigation of logical form does not proceed fully independently of subject matter, but it is nevertheless non-empirical and structural. Inference can be relevant to this investigation, especially negatively—(roughly,) we can show that two classes of judgment do not share a form by showing that their members do not exhibit the same patterns of inferential behaviour—but the availability, even in principle, of a syntactic characterisation of the valid inference patterns in which a judgment participates is not a necessary criterion for the attribution to it of a certain logical form.

I argue that Frege’s revolutionary application of *function-argument analysis* to logic plays a central rôle in his equation of the categories in terms of which to ascribe structure to thoughts with the syntactic categories needed for the systematisation of inference. Though the application is plausible when the subject matter under investigation is mathematical, I argue that function-argument analysis is ill suited to the analysis of predicative structure generally. As an illustration of this claim, following Michael Thompson’s lead, I discuss “natural-historical judgments,” a certain type of generic judgment about living things. I walk through a series of formal-semantic

proposals for generic sentences: approaches drawing on relevant quantification, notions of prototypes and stereotypes, probabilistic analyses, approaches using the apparatus of modal and situational semantics, analyses of generic sentences as nonmonotonic inference licenses, and several others. I argue that each of these founders, in one way or another, on its imposition of function-argument analysis on natural-historical judgments: for the function-analytical interpretation of natural kind terms is bound to fail to capture the dual rôles these terms play in generic and in singular sentences. The unique logical form of natural-historical judgments is not to be understood on the functional model; the categories deployed in their grasp are not explained by their use in codifying inference patterns. I conclude that the gap between a Fregean conception of logicity and the one exhibited in Thompson's work is even wider than Thompson recognises.

The view that the uncovering of logical form is a piecemeal, unprincipled affair is distinct not only from the broadly Fregean conception but also from Kant's notion of transcendental logic. I associate it, instead, with the later work of Wittgenstein. I bring out how Wittgenstein's engagement in the *Tractatus* with Frege's and Russell's conceptions of logic paves the way for his later development of the notion of grammar in the *Investigations*, in order to show how the broader conception of form I advocate has a genuine claim to logicity. I also argue against Sebastian Rödl's claim that Wittgenstein in the *Investigations* is (unwittingly, and incompletely) engaging in explorations of transcendental logic.

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Preface.

Perhaps partly due to a certain promiscuity in my interests, I have a tendency to begin, in my work, from a diversity of ideas, only later imposing on them what I hope constitutes a sort of order. When casting about, some years ago, for a dissertation topic from among my then current objects of interest, I was happy to hit on the notion of form, precisely because of the way in which it appeared to present just the sort of unifying effect that would allow me to continue to pursue as many of those interests as possible under the head of a single project. The notion of form in philosophy and logic, reflection on which appeared to pay such rich philosophical dividends in the work of, say, Kant, seemed to me to have become narrower with and after Frege's formalisation of quantification, in such a way that, though our formal apparatus for studying logical consequence had become extremely powerful, the notion of the form of a thought seemed to have had much of its philosophical interest sucked out of it. The pursuit of the question how this had happened, and whether we had in the process lost anything of significance, would, I thought, allow me to bring together several of my interests. One of these was in the work of Kant, and in particular in his application of the notion of form to the practical realm, an interest which several stimulating seminars on Kant's ethics led by Steve Engstrom produced in me. (It will be observed that Kant's name hardly appears in the sequel; the two or so originally projected chapters on his work were an early casualty of the clash between the hotch-potch approach to work I am describing and the requirements of a doctoral dissertation.)

Another was in the work of Wittgenstein, especially his *Tractatus logico-philosophicus*; reading that book with Ben Laurence was one of the more exciting and productive experiences of my graduate student career, and though my thinking about the notion of form deployed in the opening pages of that work has changed since then, I would not have embarked on this project had it not been for that experience. The years-long, always fruitful and soul-soothing conversation David Berger and I engaged in about a wide range of topics, including Michael Thompson's work on the logic of discourse about life, showed me what a genuinely philosophical exchange of ideas could be; it was this work of Thompson's that allowed me to put in its clearest form the question at the heart of this dissertation. Finally, it was in the study of truth and meaning directed by John McDowell which I pursued with Ben Laurence and Evan Riley that I began to think seriously about the notion of logical form in twentieth-century philosophy of language. I am afraid that the present work still bears the marks of the sort of fusion of initially disparate interests that the story I have just told betrays—but on the other hand, for what it's worth, this lack of unity may in fact itself present a certain kind of harmony with the conception of logical form I begin to articulate in the Coda.

My style of work, exacerbated by my familial obligations and inclinations, led me to draw on the expertise of my dissertation committee members much less than I should have. (For this reason, the now proverbial qualification to an acknowledgement of scholarly debt, to the effect that “Any errors that remain are solely my responsibility,” is especially applicable in the case of this dissertation.) But it is still true that I owe a great deal of thanks to each of them. John McDowell, beginning with the directed study I mentioned above but also whenever I gave him the chance throughout his tenure as my dissertation director, was extremely generous, as well as acute and thorough, with his feedback on my work. Michael Thompson's name, as will be seen,

is sprinkled liberally throughout this dissertation; the article I alluded to earlier, as well as several seminars he gave on his work, influenced my thinking greatly. In addition to the seminars on Kant's ethics I've already mentioned and his thoughtful responses to my work, Steve Engstrom always stood ready to give me whatever help I asked for. Anil Gupta was extremely conscientious, kind and encouraging in his feedback; the example he set of a graduate student advisor is one I aim to emulate, though I can hardly hope to meet it. Although I have not yet mentioned his name, Sebastian Rödl's work, and especially the seminar he gave at Pittsburgh in 2003, had an enormous influence on me, and I am very grateful to him for the generosity with which he indulged me when we met to discuss my research. And finally, though he was not on my committee, I must mention Nuel Belnap as perhaps the single most influential faculty member on my graduate student career, both through the many graduate seminars of his I attended and through the very generous support he gave me in the form of his Alan Ross Anderson Fellowships.

It feels to me rather odd to thank on paper the friends I made in graduate school for their "support," which is to say for their friendship (even when that friendship has included, as it often has, philosophically stimulating conversation); I would much rather acknowledge it, naturally, with the friendship of my own, in the hope that it means to them something approaching what theirs has meant to me. So I shall refrain from listing the names of the people whose companionship has been dear to me during my time in grad school. I feel this even more acutely with respect to my family, Akiko and Lia: though I owe the world to them, I can hardly imagine a less appropriate place to say so than in the pages of my dissertation.

Chapter One. Logic and the study of formal systems.

The present study is an enquiry into the notion of logical form. My starting point is a conception of that notion prevalent in twentieth-century philosophy of language and logic, according to which the inferential behaviour of sentences, or of the thoughts they express, in one way or another holds the key to their logical form. This conception is closely associated with the tradition in philosophy called “analytic.”¹

Now, there are those who conceive of logic as, fundamentally, the mathematical study of systems of the sort formal logicians devise. On this view, the “application” of these systems to natural-language discourse, though perhaps sometimes philosophically interesting, is nevertheless a merely pragmatic matter—that is to say, that some such system is applicable in a given case to some stretch of discourse is not to be understood as revelatory of “the real logical structure” of the discourse in question. On the contrary, on this sort of view, quite different systems may be applicable, for different analytical purposes, to the same stretch of discourse—

¹ I shall not engage directly with the use the phrase ‘logical form’ (or, more often, ‘Logical Form’) has acquired in recent years in empirical linguistics. As Stanley (2000: 392) explains, “Talk of logical form in this sense involves attributing hidden complexity to sentences of natural language, complexity which is ultimately revealed by empirical inquiry.” But it would not be surprising if there were connections to be drawn between that notion and the one currently under discussion: for one thing, a canonical explication of the rôle of Logical Form in transformational-generative grammar is as the level of linguistic description at which “all grammatical structure relevant to semantic interpretation is provided” (Hornstein 1995:3); Davidson (1970a: 63), many years earlier, had already remarked that “it would be strange if the structure essential to an account of truth were not effectively tied to the patterns of sound we use to convey truth,” and indeed Hornstein himself proposes understanding “semantic interpretation” in a Davidsonian framework. (Hornstein and Davidson here put the other term of the contrast in terms of semantics rather than inference; we shall consider the relation between these two in Chapter II.)

indeed, systems different enough that no single “real logical structure” attributed to that stretch of discourse could explain the various systems’ applicability to it.

Thus, consider the following expression of what at first may sound like this view:

[T]he axiomatic development of a science presupposes, apart from those things involved in the actual subject matter, three other things: (1) a language, (2) a system of deductions, and (3) a system of interpretations. This three-part structure has been called [by Church] the underlying logic of the science. In my view the aim of the science of logic is the understanding both of underlying logics and of theories which presuppose them. Logic is the science of axiomatic sciences. Logic must produce theories of ‘propositional forms’ to account for the ‘linguistic’ phenomena. It must produce theories of deduction to account for the phenomenon of deduction and it must produce theories of semantics to account for the phenomenon of reinterpreting languages and satisfying or falsifying sentences.... (Corcoran 1973: 27)

Here, Corcoran places ‘propositional forms’ and ‘linguistic’ in scare-quotes because he is speaking not about natural-language contexts but about the formal languages used in the axiomatic treatment of a science. This seems like a perfectly clear expression of the view of logic described above, as the mathematical study of formal systems and their formal interpretation. But now consider what the same author writes a couple of pages later:

The construction of mathematical models not only increases clarity and precision but it also relieves two pressures—the pressure to be right in every detail and the pressure to give an account of the ontological status of the subject. Today the value of idealized models is widely accepted and hardly any of the current logicians feel pressure to decide the relation between the logical and the mental, to give an account of propositions, to explicate the ground of logical consequence, etc. Incidentally, with the possible exception of modal logics, hardly any worthwhile logical doctrines seem to have started from any analysis of ordinary language or from analysis of reasoning outside of mathematics and science. The reason is not hard to find: if one wants to study a certain phenomenon then one seeks places where the phenomenon occurs repeatedly and clearly. The reasoning which occurs in mathematics and science is frequently free of the emotionalism and sectarianism which infects other areas. (Corcoran 1973: 29-30)

This passage betrays the thought that there may be more to logic than *merely* the study of formal systems and their interpretation. Indeed, the last paragraph quoted here would appear, according to the official definition of ‘logic’ from the first quotation, wildly off-topic: for if logic really simply *were* “the science of axiomatic sciences,” then there would be absolutely no reason to have expected any “worthwhile logical doctrines” to have arisen from the “analysis of reasoning

outside of mathematics and science.” But the very idea of *modeling* brings with it the idea of something *modeled*: and if logicians do not currently “feel pressure to decide the relation between the logical and the mental, to give an account of propositions, to explicate the ground of logical consequence, etc.,” it need not be because such questions have no sense.

And indeed, the idea that the notion of logical form might apply to something beyond formal languages is not at all unfamiliar. For one explicit example:

... much of the interest in logical form comes from an interest in logical geography: to give the logical form of a sentence is to give its logical location in the totality of sentences, to describe it in a way that explicitly determines what sentences it entails and what sentences it is entailed by. (Davidson 1970b: 140)

Davidson is here speaking of the logical form of *natural-language sentences*; specifically, sentences about actions. Now, Davidson quickly adds that the notion of logical form at play here is “relative both to a theory of deduction and to some prior determinations as to how to render sentences in the language of the theory” (*ibid.*); but he does not take this to impugn the very idea of ascriptions of logical form to such sentences, especially since he considers the theory of deduction he favours, classical first-order quantification theory, to be “a good theory.” And Gareth Evans (1976) has argued that we ought to demand, and can provide, a more robust—more “transcendent,” as he puts it, in Quine (1970)’s sense—conception of the structure in virtue of which formal inference is valid. We are to sort expressions into semantic categories according to their inferential properties, but we are to posit a new such category only when we can assign to it an “underlying real essence” (a type of entity, such as a set, an object, a function of a particular type, or whatnot) which explains the logical behaviour of the expressions in that category.

For one more example, consider the following words from Anil Gupta’s (1980) study of common nouns:

My aim in this essay is to explore the logical role of an important class of concepts—concepts that are expressed by common nouns (“sortal concepts”)—in a fragment of our conceptual scheme. It seems to me that this exploration is essential not merely for purely logical purposes but for certain philosophical and linguistic purposes as well.... And it provides material that I think will lead to a better understanding of the relationship of logic to ontology.... (Gupta 1980: ix)

It is the logical rôle of the concepts in question “in a fragment of *our conceptual scheme*” in which Gupta is interested: not merely the logical rôle the corresponding concepts play in the systems he designs to model that scheme. He goes on to say that “there are important logical and semantic differences between common nouns and predicates” (*ibid.*, p. 1) which standard systems of quantified modal logic fail to capture. Again, his language is unequivocal: common nouns of natural language have logical properties, and a given formal system is inadequate to them as long as it fails to model those properties.

Now, it may be, in fact, that a great many logicians, as well as (perhaps fewer) philosophers of logic and language, do have views closer to those articulated in our first quotation from Corcoran. I do not mean to claim that such views are incoherent, but only to draw attention to the way in which even some of their clearest advocates can betray traces of the opposing view.

And it is worth adding that the idea (which Beall and Restall (2000) call “widespread” among philosophers of logic) that there is “one true logic,” like the idea that (one) logic is normative for thought, appears to presuppose something like the view under discussion, that the proper object of the study of logic is not merely formalisms and their formal interpretations but the language or thought of which they are models. Indeed, even the acknowledgement of certain kinds of logical pluralism is compatible with this view, as long as one believes that each of the plurality of logics is true of, or normative for, its respectively appropriate field of application.

The present dissertation will not address head on the question which of these views is justified.

The purpose of the preceding discussion has just been to show that the idea is not entirely alien

to contemporary logic and philosophy of logic and language that the study of logic is, at least in part, the study of *thought* (or at least of language), and not merely the investigation of the formal models of language which, after all, are *models*.²

But it is noteworthy that each of the exemplars I have cited of the view that there is more to logic than formal models has nevertheless made quite clear that it is through the use of those formal models that we are to carry out our logical investigations. Thus Davidson holds that one gives a sentence's logical form by giving a deductive theory for (at least a part of) the language of the sentence—and further, though the ascription of logical form to sentences will therefore be relative to that theory, first-order logic is “a good theory” for this purpose precisely because (thanks to Tarski) we have a formal *semantics* for it, and that is to say, if we can paraphrase a sentence in quantificational idiom, we can *explain* why it has the inferential properties it has. Evans, as we saw, made it quite explicit that a necessary condition of entitling oneself to a claim about a sentence's structure in the relevant sense was, in effect, to locate it in a formal semantic theory. And Gupta, too, though he does give philosophical arguments for his conception of the distinction between common nouns and predicates, places at the centre of his treatment the “construct[ion of] systems of quantified modal logic in which a categorial distinction is made between common nouns and predicates.... [such that t]he syntactic and semantic behavior of common nouns in these logics is quite different from the behavior of predicates” (1980: 4).

As long as (in accordance, for instance, with Corcoran's official line) we understand it as a *definitional* matter that logic's proper object is formal systems, it will emerge as analytic that

² This can be taken as a modest corrective of Rödl's (2005: 8) claim that “‘logic’ in the analytic tradition now only designates the science of formal calculi.” Of course, Rödl's main point is that “the analytic tradition” has not *entitled* itself to a conception of logic beyond this; that some scholars nevertheless employ such a conception does not tell against *this* point.

formalisability is constitutive of the matter of logical study. But on the opposing view that we have been discussing, there is room for a question: what is the relation between formalisability and the study of logic? What rôle do formal systems play, and might logic perhaps outstrip them? Of course, it is still open, on this conception, to answer the last part of the last question negatively: it may be that the structure of thought (as we might call the subject matter of logic on this broader conception) *can* adequately be captured by a formal system or systems. The point is that on this second conception of logic, but not on the first, the question is an intelligible one.³

It is this question with which the present dissertation will be more directly concerned. Supposing that there is sense to be made of the idea that the notion of logical form has applicability to language or thought, I shall begin, in Chapter II, by describing in greater detail the conception of logical form that associates it with formalisability, of which we have considered the cases of Davidson, Evans and Gupta as illustrations, and which is in any case the mainstream view (at least among those who do not conceive of logic as confined to the study of formal systems to begin with). I shall explain how it arises in tandem with Frege's development of his own formal treatment of inference.⁴ Next, in Chapter III, I shall argue, following Michael Thompson's lead, that that mainstream understanding cannot give a satisfactory account of the structure of natural-historical judgments. In Chapter IV, I shall make a modest attempt to cast doubt on a proposal of Rödl's to understand the sense of "logical form" at issue in Thompson's work, as well as the idea of grammar as it is deployed in Wittgenstein's *Philosophical investigations*, as having its home

³ There are those who do not see a significant disanalogy between formal and natural languages (Quine is a notable instance; for one example, his discussion of grammar in his (1970) evinces this attitude). I shall (henceforth silently) take this view to be of a piece with the conception of logical form that associates it with formalisability, since it holds natural languages (or anyway natural languages submitted to a spot of regimentation) to be, in effect, formalised already. (Thanks to Thomas Ricketts for reminding me of, and pushing me on, this.)

⁴ I shall not lay much emphasis on the historical claim that the conception originates with Frege, mainly because I shall not do any of the archæology that would be required to substantiate such a claim. But given the connection between the conception and the idea of a begriffsschrift, the historical claim does have a certain plausibility.

in the Kantian notion of transcendental logic. In Chapter V, I shall argue that the trajectory from Wittgenstein's early work to his later constituted not so much a radical break with "Fregeanism" after the *Tractatus* as a development of a nonFregean conception of logical articulation already present, if muted, in that work. And finally, in a Coda, I shall make some modest remarks about the conception of logical form that emerges from these reflections.

Chapter Two. The idea of a begriffsschrift.

Contents:

- §1. The begriffsschrift as a system of purely syntactically definable inference.
- §2. Inference and categorial structure.
- §3. One mode of combination.
- §4. Predication as functional.
- §5. Suspicion of the functional theory of predication.
- §6. Response to the suspicion.

§1. The begriffsschrift as a system of purely syntactically definable inference.

Begin with the fact that Frege’s overarching project was to answer the question on what rested the truths of arithmetic. His purpose in devising his begriffsschrift¹ was to clarify fully the chains of inference involved in mathematical proof, in order to expose every unproven premise and every mode of inference, so as to determine whether (as the claim that arithmetic depends on intuition seems to assert) anything on which arithmetic rests is peculiar to arithmetic, or whether all the premises and modes of inference necessary for the successful implementation of arithmetic are in fact fully general, that is, applicable to any subject matter indifferently.

The way in which the begriffsschrift “clarifies fully the chains of inference” in question is by rendering inference *mechanical*, in the sense that whether a step in such a chain is correct is

¹ As this word cannot be alleged to have entered the English language, it really ought to be in italics—but then, since German nouns are capitalised, it would be indistinguishable from the title of Frege’s first book. For the sake of disambiguation, then, when using it as a common noun I shall proceed typographically as though the word were thoroughly familiar to speakers of English (as it surely is at any rate to my readers).

easily—mechanically—checked.² He does this by defining his inference rule(s) in purely syntactical terms. As he puts it in his *Foundations of arithmetic*:

[My *begriffsschrift*] is designed to produce expressions which are shorter and easier to take in [than those of natural language], and to be operated like a calculus by means of a small number of standard moves, so that no step is permitted which does not conform to the rules which are laid down once and for all. It is impossible, therefore, for any premiss to creep into a proof without being noticed. (1884: §91)

What Frege flags as his single inference rule, *modus ponens*, is implemented in his system in such a way that an application of it involves merely, as it were, copying and pasting from formulae already derived, having inspected them to ensure that their parts match appropriately.³ Similarly, Frege's (implicit) rule of substitution is equally (if a little more complicatedly) syntactically characterisable.

That Frege's system only employs these modes of inference is not intended to suggest that *modus ponens* and substitution are somehow the only real or correct forms of inference, of course; Frege himself frequently draws attention to the decisions he has made in setting up his system, and (though he sometimes gives (often merely practical) reasons in support of his decisions) to the fact that he could just as well have made them otherwise. (Thus he discusses the flexibility involved in the choice of primitive symbols at, for instance, (1880-81: 37); in the choice of axioms, at *Begriffsschrift* §13; and as for the inference rule of *modus ponens* itself, he places emphasis on the following entire sentence: "This restriction to a single mode of inference, however, is in no way intended to express a psychological proposition, but only to settle the

² Of course, it emerged from later work in metalogic, especially Church's and Turing's (drawing on Gödel's), that neither Frege's *begriffsschrift* nor any even first-order system of relational logic is successful at rendering inference mechanical in the sense of providing an effective procedure for the *generation* of a proof of the validity of a given chain of valid inference. But though Frege was innocent of this work, it is notable that he did not make the claim for his *begriffsschrift* that this later work was to show unrealisable: on the contrary, Frege wrote: "our thinking as a whole can never be coped with by a machine or replaced by purely mechanical activity" (1880-81: 35).

³ The observation that the two-dimensionality of Frege's system particularly suits it to the copy-and-paste approach is Robert Brandom's. Van Heijenoort makes a brief remark to the same effect in his introduction to Frege's *Begriffsschrift* (1967a). Of course, the *general* point about the "mechanical" nature of formal systems will apply equally to linear systems.

question of formulation to maximize effectiveness” (*Begriffsschrift*, §6). It is true that he argues that all other modes of inference can be reduced to it; but on the other hand, he does not argue explicitly that the same is not true of any other candidate mode of inference. In any case, it is clear that it is in the interests of (mere) *perspicuity* that he refrains from building other modes of inference into his system—not because they would be any less correct than *modus ponens*.) What he emphasises is that it is important for his purposes that the modes of inference be *few*; and it is likewise crucial for those purposes that they be mechanical.

The description of Frege’s project in the paragraph before last was, in fact, at the same time a (rough) description of the very notion of a formal system. Though the syntactic manipulations necessary for carrying out inferences in some formal systems can be substantially more complicated than those required in Frege’s *begriffsschrift*, every formal system begins with a recursive characterisation of the formulae of the system, on the basis of which are then laid down the inference rules of the system—where by “on the basis of which” is meant that the rules are defined purely syntactically.

(I say “rough” because, though van Heijenoort (1967a), for instance, credits Frege with the invention of the idea of a formal system (and see also Dummett (1973/1981: 82), where the same view is taken for granted), Frege’s system does not quite fulfil the now standard definition of this idea.⁴ For Frege’s system allows the introduction of new symbols through *definitions*, whereas a formal system’s recursive characterisation of the notion of ‘formula of the system’ is to be formulable in terms of vocabulary laid down once and for all. However, it should be clear that permitting the definitional introduction of new notation does not threaten the mechanical checkability I have been discussing: for one can always simply cut out a defined symbol and

⁴ I owe this observation to Thomas Ricketts, who made it to me in conversation.

paste its definiens in its place (and Frege emphasises the importance of this at §24 of *Begriffsschrift*); once one has done this for every defined symbol in a proof, one will have a parallel proof framed in the original vocabulary of the system. In addition, the typical requirement on a formal system—that there be an effective procedure for determining whether a given string is a formula—is left intact even when the expansion of the alphabet by definitions is permitted, so long as the definitions themselves are available to the checker. If, when all defined symbols in a string have been eliminated in favour of their definientia as described above, the result is an admissible formula, then the original string will be held to inherit that admissibility.)

In short: Frege was the inventor of the formal system (or close enough), and its purpose, for Frege, was to represent, with no gaps—no implicit appeals to tacit premises or modes of inference—and in a way mechanically characterisable and checkable, the inferences made in arithmetic. (Frege of course envisioned the further application of his methods to fields besides arithmetic; recall the passage in the Preface to *Begriffsschrift* in which Frege mentions the calculus, geometry, mechanics and other fields as potential future objects of *begriffsschriftliche* systematisation. However, arithmetic was clearly his first, and a perennial, concern.⁵) The validation, in the *begriffsschrift*, of a given chain of inference can be carried out purely by means of the rule-governed manipulation of signs.

⁵ This idea, of expanding the *begriffsschrift* further by adding to its material vocabulary in order to apply it to fields of enquiry beyond arithmetic, might appear to generate a greater obstacle to viewing the *begriffsschrift* as formal in the standard sense than the definitions discussed above, since this sort of introduction of new vocabulary would presumably not proceed by definition in terms of existing vocabulary. But it would probably be better, and would in any case be natural, to think of each such enrichment of the vocabulary as resulting in a new system with different primitive symbols, rather than thinking of both the starting-point and the result as the same system and worrying about its formality. In any case, the system presented in *Begriffsschrift* includes no provision for this kind of material enrichment of vocabulary, despite the remarks in the book's front matter, so this obstacle is not a reason to doubt the formality of the system of *Begriffsschrift* itself. Both §24 of *Begriffsschrift* and §27 of *Grundgesetze* present definitions as, in the words of the latter, the introduction of “new signs by means of signs already known.”

§2. Inference and categorial structure.

Near the surface of the discussion of the previous section, though perhaps left somewhat implicit, was the fact that a *begriffsschrift* is used to represent propositions already expressible in another system of representation. This is not to deny that someone might express in *begriffsschrift* a proposition never before expressed in the course of producing a proof of some (say) arithmetical claim (or even, conceivably, as the claim to be proven); indeed in such contexts there surely appear in *Begriffsschrift* *begriffsschrift* propositions whose ordinary-language (or better, arithmetic) equivalents had never before been written down. But Frege himself recognises, indeed emphasises, the awkwardness of his notational system for the purposes of creative proof (cf. the analogy with the microscope in the Preface to *Begriffsschrift* (p. V)). In any case, all I want is this point: the *begriffsschrift* is not, in the first instance, a means of expressing new ideas—ideas, say, inexpressible in ordinary language—but a better means (for Frege’s limited purposes) of expressing the same ideas already expressible by other means.

But this of course requires some attention to the question of the *application* of the notational system: of the relation between the (syntactic) form a thought takes when expressed in the *begriffsschrift*, and the form it takes when expressed, say, in ordinary language, or in the standard notation of arithmetic.⁶ There is at any rate one notion of *element* implicit in the grammar of the *begriffsschrift* (as our discussion of the idea of a formal system implies, through its reference to recursive characterisation). And there is an analogous notion of *element* which comes with ordinary language (or with arithmetic notation): namely, the notion of a *word* (or

⁶ It will be noted that I am here taking for granted, what is in fact a quite contentious claim, that to natural-language and *begriffsschrift* sentences there correspond *thoughts* into whose structure we may enquire. It helps a bit to point out that I am currently engaged in Frege exegesis, and it is anyway uncontroversial that *he* assented to this claim. (But it is also true that I go on, in this dissertation, to take the results of the reflections contained in this chapter to apply to conceptions beyond Frege’s own; and I cannot deny that the assumption this footnote records will remain undischarged.) If you are less bothered by the term ‘proposition’, please feel free to substitute it.

symbol).⁷ But the grammar of ordinary language comes with no guarantee that its elements (in that surface-syntactic sense) would be suitable for the development of an inferential system such as Frege envisaged; indeed it is precisely his claim that it is not. (Consider again the analogy with the microscope; compare also Frege’s remarks on the subject-predicate distinction in contrast with the function-argument distinction, *e.g.* at *Begriffsschrift* §3.) And likewise even for the standard notation of arithmetic. (Thus Frege points out (of the theory of magnitude more generally) that the distinction between constant and variable is not consistently maintained, *e.g.* as concerns the symbols 1, log, sin, Lim: *Begriffsschrift* §1.) Meanwhile, of course, the grammar of the *begriffsschrift* *is*, precisely, meant to serve as a framework for such an inferential system: as we saw, the idea behind Frege’s *begriffsschrift*, and the general idea of a formal system, is that of a notational system in terms of whose *grammar* the modes of inference are defined. Again, since (for the purposes of making the expression of chains of inference gap-free and mechanically checkable) the modes of inference will be (as discussed above) of a *copy-and-paste* form, it will be appropriate for the *begriffsschrift* to render with a separate syntactic element each part of a thought (using ‘part’ in a so far metaphorical and unexamined way) that might be relevant to the thought’s inferential behaviour. In short, it can seem, at least, as though, when translating a familiar thought from ordinary language, or arithmetic notation, into one’s *begriffsschrift*, one faces the question of what elements that thought consists.

(By the way, this conception of the “parts of a thought” need not violate Frege’s own “context principle”—as should be plain from the fact that it echoes Frege’s own language:

It is astonishing what language can do. With a few syllables it can express an incalculable number of thoughts, so that even a thought grasped by a human being for the very first time can be put into a form of words which will be understood by someone to whom the thought is entirely new. This would be

⁷ However, as Anscombe discusses in her (1981), the division of (spoken) natural language into words is not simply to be taken for granted.

impossible, were we not able to distinguish parts in the thought corresponding to parts of a sentence. (“Compound thoughts,” p. 36)⁸

We are after all envisioning the train of thought here sketched as accompanying the creation of a *Begriffsschrift*, that is, of a whole *system* of notation. The point is just that once one has available a (quite possibly *systematically*, that is holistically, motivated) parsing of a thought, one can conceive of each element’s contribution to that thought independently, just in the sense that, for example, one can recognise that element again as it appears in other thoughts having (by the lights of the system) no elements but that one in common with the first. –And ‘recognise that element again’ may well just mean: recognise that the thoughts in question are inferentially related, through their possession of that shared element.)

A possible objection to this jump—to the jump from the idea of establishing a connection (however holistic) between the syntactic elements of one notational system and those of another, to the idea of positing elements *in the thoughts themselves*, in abstraction from their expression in any given notational system—may arise from the idea that, after all, the need to *systematise inference* does not have to amount to the need to systematise *all* inference. Indeed, this is not an unfamiliar attitude in the philosophy of logic. It finds clear expression in, for example, Belnap, Perloff and Xu’s (2001) discussion of the relation between their proposal for the logic of action sentences and Davidson’s (1966). The two proposals appear to be quite incompatible, in the sense that one could not construct a “bigger system” for action sentences true at once to both proposals. (Contrast, for example, the sense in which sentential logic, although it posits no internal structure in the atomic sentences of which it treats, is nevertheless plainly compatible with predicate logic—plain because the latter after all contains the former.) However, rather

⁸ This is of course a different reason for introducing the idea of parts of a thought from the one under discussion above.

than feeling obliged to argue for the falsity of Davidson's proposal (though they do offer some considerations in that direction as well), they write: "[Davidson] objects that modal constructions do not solve his problems, for example variable polyadicity. We concur, noting only that we are concerned with different problems..." (2001: 79). One might, that is, hold that the project of "systematising" inference needn't be *global*; indeed the view expressed by Belnap, Perloff and Xu suggests that it *can't* be global. On this view, we can *create* a system of structures designed to track a certain sort of inferential behaviour exhibited (perhaps only somewhat fuzzily) by natural-language sentences; but doing so has no implications for the structure of those sentences themselves—nor for the structure of the thoughts expressed by them—since, after all, if we had wanted to track some *other* sort of inferential behaviour exhibited by those same sentences, we might have had to create a different and incompatible system.

As I say, this is not an unfamiliar view; but nor is it universally held, nor again is it, in any case, Frege's.⁹ As we saw, Frege's purpose is to establish for certain on what arithmetic rests; and to that end, but also more generally, he aims with his *begriffsschrift* to render inferences *fully explicit and gap-free*. Now, a "partial" system (in the sort of sense we have been considering) may of course be enough to accomplish this task for *some given inference*: as, for instance, sentential logic suffices for establishing the validity of certain classes of inference. But of course the idea of a *begriffsschrift* as Frege envisions it is the idea of a system which enables the

⁹ It is perhaps telling that pressure is put on it by the very considerations Belnap, Perloff and Xu raise in their more positively anti-Davidsonian remarks: thus they point out that, when Davidson proclaims his intention to examine what rôles the parts of simple action sentences have with respect to those sentences' inferential properties, "[h]alf the compositional problem has been left out" (p. 78)—that is, he neglects the question how such sentences themselves embed in larger contexts; and later, when he does address this latter question to some degree, they jibe at the fact that his remarks bear little relation to his overall account, "since generally in compositional semantics a view about the logical form of a 'part' of a certain kind both constrains and is constrained by a view about the logical form of an expression that embeds just that kind of part" (p. 79). This *sounds* rather like a demand for a "global" conception of the semantic project. (But perhaps their idea is simply that *any given account* must be as it were harmonious, not that it must purport to capture everything about the phenomena.)

explicit and gap-free representation of *all* (formal) inference; for after all, if it were “partial” in the above sense, an enquirer into the basis of arithmetic, for instance, might be left wondering, about a given inference not sanctioned as formally valid by the *Begriffsschrift* in use, whether it might not after all still *be* formally valid, but on the basis of some inferential property not captured by the present *Begriffsschrift*.¹⁰

But how can one be certain that one’s system “enables the explicit and gap-free representation of *all* (formal) inference”? Van Heijenoort (1967b) argues that Frege’s own “universalist” conception of logic prevents him from framing a question like this, at least in the way that contemporary metalogic would: he cannot, that is, hold his proof theory up against an independently conceived model theory to determine whether it is complete. Instead, Frege must proceed “experimentally”, by canvassing the modes of inference actually used in arithmetic (for instance) and determining whether he can reduce them to those sanctioned by his system (or whether he can expand his system to encompass them while holding on to the conception of his system as a system of *logical* inference).

Now, though van Heijenoort is certainly right that Frege lacks the contemporary conception of model theory (whose initiation van Heijenoort attributes to Löwenheim)—and that this lack is not merely accidental, but is essential to his conception of the quantifiers (in turn essential to his conception of logic) as always ranging over *everything there is* (so that the idea, central to model

¹⁰ Could it be that arithmetic rested on some *proper part* of formally valid inference, and that Frege would content himself with capturing that part of it? But compare the passage from the Preface to *Begriffsschrift* alluded to above, where he gestures at the application of the *Begriffsschrift* to fields besides arithmetic. (Note that, though he foresees the need for “a further development of the symbolism” to facilitate this application, he is not envisioning a development of the *logical* structure of the symbolism, nor (hence) of the range of types of formal validity it sanctions: for ‘further’ here appears to hark back to the previous paragraph, where he says that, to accommodate geometry, “[o]nly a few more symbols would have to be added *for the intuitive relations that occur here*” (*Begriffsschrift*, Preface, p. VI, emphasis added).)

theory, of “varying the domain” is unintelligible to him)¹¹—it might be pointed out that Frege does, nevertheless, have what can perfectly well be called a formal semantics.¹² Thus (to give a brief and very partial sketch, or rather, a pair of examples) it is a famous doctrine of his that singular terms refer to objects (when they refer at all, as they do in a properly scientific notation) and predicates to concepts; and that this distinction between object and concept, located as it is in the realm of reference, is “founded deep in the nature of things.”¹³ Herein, indeed, lies the core of the solution he proffers to the problem of predication: concepts, being a special case of functions, are “unsaturated” (*ibid.*, p. 6), and are completed by objects to yield a unified whole.

But where does this doctrine come from? How does Frege know that the analysis of sentences into names and predicates, referring to objects and concepts (indeed concepts of varying levels and taking varying numbers of arguments), yields a correct account of the unity of those sentences? One answer to this question places primary emphasis back on Frege’s attention to inference. Thus Thomas Ricketts (1985, 1986) has argued that Frege distinguishes between singular terms and predicates on the basis of the differences in their respective rôles in inference: from a sentence containing a singular term one can in general infer another sentence, derived from the first by replacement of the singular term by another co-referring term in accordance with Leibniz’s Law; and these same terms play a distinctive rôle in inferences concerning generality. Given this inferentially grounded account of a singular term, one can then formulate a corresponding account of a predicate: it is that which is left when a singular term is elided from

¹¹ and this in turn is of a piece with Frege’s thinking of the formulae of his *begriffsschrift* as meaningful rather than as schematic (hence the title of van Heijenoort’s article). I am setting aside many such details in the text.

¹² As Dummett notes: “Frege did not explicitly state the modern distinction between the semantic (model-theoretic) and the syntactic (proof-theoretic) treatments of the notion of logical consequence: but it is implicit in his writing. . . . [for in addition to the formal system,] the semantic explanations of the sentences of the formalized language [are] set out, clearly separated from the formal development, in German in the accompanying text” (1973/1981: 81-82).

¹³ “Function and concept” (1891: 31). In the sentence from which I have quoted, Frege is actually speaking of the distinction between first- and second-level functions; but it is manifest that his view carries over to the distinction I discuss in the text.

a sentence. Meanwhile, Dummett (1973/1981: Ch. 2) has argued that the metaphor of “unsaturatedness” favoured by Frege when speaking of the contrast between functions and objects has its home in the notion of a “complex predicate”, which is needed not to account for the internal structure of atomic sentences but for the behaviour of quantified sentences built up from them.

It is of course undeniable that inference is of great and perennial concern for Frege; as we have seen, from the beginning of his first published work he emphasises that the design of his *Begriffsschrift* was controlled always and only by the consideration that it be suited to the perspicuous representation of inferential behaviour. And there seems to be no reason to suppose that this concern was sidelined in his later work (where his attention to semantics becomes more prominent). Nevertheless, Ricketts’s reading of Frege is not uncontroversial. It has been observed, first of all, that distinguishing between singular terms and predicates on the basis of their rôles in inferences involving generalisation, instantiation and Leibniz’s Law, for example, is not as simple as Ricketts suggests.¹⁴ A perhaps deeper worry concerns the imputation to Frege of a contentious answer to the question of the relative priority of inference and semantics. For if Ricketts is right, a prior conception of correct inference is what enables Frege to develop a posterior conception of the semantic values of expressions, together with a conception of their truth.¹⁵ But one may well wonder whether the notion of *correct inference* is available to one who lacks the notion of *truth* and hence of *truth preservation*. One may wonder, that is, whether it makes sense to imagine a thinker who has, first, a conception of the inferential relations in

¹⁴ Thus *e.g.* Dummett himself, in “Proper names” (1973/1981: Ch. 4), goes to extreme lengths to elucidate Frege’s notion of a proper name along the sort of lines Ricketts has in mind here, and in the end leaves the account a little schematic (though he does conclude that “the Fregean notion of a proper name is in principle capable of being supplied with precise criteria which are formal and linguistic” (p. 80)). (Thanks to M. Valaris for drawing my attention to the relevance of this reference here.)

¹⁵ This may not be quite fair to Ricketts; see the second paragraph of footnote 18 below for a qualification.

which her judgments stand to one another, and only later extracts from this a conception of *objects* as that about which her judgments are true or false. One might argue, rather, that to engage in the practice of *judging* is to take one's judgments to be *true*, and to recognise that (in general) the truth or falsity of one's judgments is independent of the fact that one is making them. But this is to say that they are *objective*, that is, that they are *about objects*.¹⁶ And one might argue, in opposition to the view Ricketts ascribes to Frege, that the inferential relations in which one's judgments stand to one another derive from these prior facts about their content: good inferences are truth-preserving. (Indeed, the analogue of this view in the field of formal logic appears to be the default position, as witnessed by the use of the terms 'soundness' (or 'consistency') and 'completeness' to describe how well a proof theory for a formal language measures up to the language's semantics.) If all this were so, we might find room for a sense in which Frege's logic—the systematisation of inference constituted by his *begriffsschrift*—is accountable to semantics after all (even if not to model theory): for if Frege has this independent conception of the structure of thoughts, he may take it to be something to which he can hold up for comparison the structure his *begriffsschrift* itself imputes to thoughts on the basis of their inferential behaviour.¹⁷

However, confronted by these competing claims of priority, it ought to become clear that there is a third alternative. If, on the one hand, to infer is to infer *correctly*, where that involves (at a

¹⁶ This move may appear to consist in nothing more than word play; and no doubt it is too quick. It would take work to show that the idea of an *object* (in anything like the sense in which Frege means the word) is presupposed by the very idea of objectivity. (Indeed Ricketts himself argues, in his (1986), that Frege's notion of objectivity, too, is prior to his semantic conceptions.) I mean the remark in the text just as a programmatic indication of an alternative motivation for a categorial analysis, not as a full spelling-out of such a motivation.

¹⁷ One might here compare Evans's (1976) suggestion, which we discussed in Chapter I, that we are only entitled to treat differences in inferential behaviour among classes of expression as *structurally* grounded, and hence to treat the classes as distinct semantic categories, when we can find a distinct "underlying real essence" (a set, an object, a function of a particular type, or some such) for each class that can *explain* the inferential differences. Similarly, one might think of Davidson's (1970) complaint against Cargile's calculus for action sentences, to the effect that he gives (indeed can give) no *semantics* for it, and hence cannot *explain* its correctness even *qua* inferential calculus.

minimum) truth preservation, at the same time (it might be held) one cannot be said even to have *understood* a sentence, much less to have judged of its truth, if one has no inkling of its inferential connections to other sentences. In other words, rather than affirming the priority of inference over semantics or *vice versa*, one might hold inferential and semantic notions to be equally basic and interdependent.¹⁸

For my purposes here, it is not necessary to resolve this issue.¹⁹ We began, after all, with a discussion of the views of Frege, not of the truth of the matter. But further, it's not even necessary for me to resolve the question how best to read Frege: for what is in any case common to all three of these views of the relation between inference and semantics is the idea that the (formal) inferential properties of judgments are tightly connected with—that is, stand in something like a one-one relationship with—their semantic structure.

Now, if I have a conception of the semantic structure of a sentence, or of the thought a sentence expresses, as settled by its (formal) inferential behaviour—whether I mean by that 1) that the inferential behaviour is determined by, and hence acts as evidence of, that structure, in principle graspable independently, or 2) that the categories in whose terms I articulate the notion of structure derive their very intelligibility from the prior notion of inference, or finally 3) that the categorial structure of a sentence is neither graspable independently of inferential notions nor

¹⁸ Here compare John McDowell's (2005) critique of Robert Brandom's *Making it explicit* (1994). Brandom champions the former priority claim over the latter, and claims to find supporters of each among major authors in the history of the philosophy of logic; but McDowell argues that Brandom misses the third possibility alluded to in the text, and that this in turn distorts his readings of some of those authors.

To be fair, by the way, it is perhaps more charitable to read Ricketts's reading of Frege as in this camp than as in the "inferentialist" camp with Brandom. Ricketts's central concern is perhaps not so much with the priority of inference over semantics as with the priority of inference over *ontology*; and indeed Ricketts tends to group assertion, judgment and truth together with inference, and in contrast with ontology, as notions hanging together in an interdependent system in Frege's thought. (But since the ontology in question is precisely that in terms of which Frege articulates his semantics, it is tricky to keep them separate in the way that this concession to Ricketts would seem to require: hence my use of Ricketts's work in the text.)

¹⁹ Indeed, as will emerge, I'm not convinced that there *is* a straightforward resolution to be had—at least not in the terms here laid out.

simply constituted by them, but is coeval and interdependent with them—a structural description of a sentence on that conception will at once yield an account of its inferential behaviour.

Frege’s conception of a perspicuous notation, a *begriffsschrift*, is as one which at once represents perspicuously sentences’ inferential potential and captures their categorial structure.

The bare idea that inference is related to structure is of course not new with Frege. What I want to suggest is new with Frege is the thought that a complete, *systematic* treatment of inference, of the sort that we have been exploring, will at once yield full insight into propositional structure.

This claim has a very close affinity to a thesis of Sebastian Rödl’s, who claims that “Frege’s central thought was that a certain deductive order characterizes thinking as such” (2005: 8). In what has preceded, I have been trying to give an account of Frege’s arrival at this “central thought”. We shall return to Rödl’s thesis in Chapter IV.

§3. One mode of combination.

Now consider the sentences ‘Caesar is dying’ and ‘Caesar was dying’: consider, in particular, trying to regiment them in a *begriffsschrift* of the sort we have just explained Frege’s to be. Now, it seems intuitively clear that these two sentences share another common element besides the one whose presence is indicated by ‘Caesar’: *viz.*, the one whose presence is indicated in each case by a conjugation of the verb ‘to die’. This is of course contestable; that is to say, one could insist that there are reasons for finding no common element indicated by both ‘is dying’ and ‘was dying’. In particular, in view of the connection we have seen between inference and categorial structure underlying the notion of a *begriffsschrift*, we may believe ourselves forced to a view such as this if we cannot devise a satisfactory regimentation of the inferential behaviour of these sentences that imputes to them this common element. But the natural hypothesis with which to begin is the one just recorded; and after all it would be a quite drastic measure to deny it, since

there *do* after all appear to be what look like *structural* inferences hanging on the reappearance from one of these judgments to the other of a common element. (For instance, it would seem that the following inference form is valid: $(\phi)(F)((x)[\text{whenever } x \text{ is } \phi\text{'ing, } x \text{ is } F] \rightarrow (y)(y \text{ was } \phi\text{'ing} \rightarrow y \text{ was } F))$.) Let us therefore work, for the moment, with the supposition that ‘is dying’ and ‘was dying’ indicate the presence in the respective judgments of a common element.

Now, though in our example the string ‘dying’ appears in both of the sentences under consideration, in many languages it is by means of a difference in inflection that it is indicated that the two elements we are supposing to be common to both of these sentences are nonetheless combined in different ways in each: and that is to say that there is no one string of symbols that invariably indicates the presence of the second element under discussion. But it would clearly be no less plausible *prima facie* that there is indeed a common element. A begriffsschrift meeting Frege’s desiderata—one in which correct inference can be delineated by purely syntactic means—would therefore incline us to rewrite sentences of such languages in such a way as to represent the second element, too, consistently by a single symbol or string, so as to fit it for such syntactically definable manipulation.

(I do not mean to imply that the idea of a notational system in which inference is definable purely syntactically is flatly incompatible with the phenomenon of inflection (for instance): since, for one thing, an inflection can itself, presumably, be characterised by purely syntactic means. (The presence of irregular verbs makes this difficult, but surely not impossible; and in any case, since the idea of a language with no irregular verbs is manifestly consistent, the fact (if it were one) that irregularity might rule out the possibility of characterising inflection syntactically would not show that inflection itself is directly incompatible with the idea of a Fregean

begriffsschrift.) But I want to bring out as clearly as possible the consequences of treating sentences such as these as containing more than one element in common, and abstracting from the phenomenon of inflection helps to do this. In any case, since (for the reasons already canvassed) Frege wanted to keep his modes of inference as simple and few in number as possible, it is plain that he would prefer a system in which each element were represented consistently by a single symbol over a system countenancing a special and highly complex characterisation of substitution tailored to the irregularities of German or English.)

But we will certainly not represent both sentences as, say, ‘Caesar—dying’—that is, using identical strings of symbols—since, given that a begriffsschrift is such as to characterise inference syntactically, this would force the system to treat the two sentences as inferentially indistinguishable, but they are not so (for from ‘Caesar is dying’, but not from ‘Caesar was dying’, one can infer, for instance, ‘Caesar is dying’²⁰). A begriffsschrift requires a further symbol, or at least some sort of typographical distinction²¹, to mark the difference. We might thus write the two sentences as, for instance, ‘Caesar / dying’ and ‘Caesar \ dying’, or perhaps just as ‘Caesar^{dying}’ and ‘Caesar_{dying}’.

²⁰ If I report at 4:00 that Caesar was dying when I visited him at 3:00, I do not thereby commit myself to the claim that he is now dying; he may have already died (or alternatively the blood-letter may have since arrived and cured him).

²¹ The idea of employing a typographical distinction may remind us of Sellars’s “Jumblese”. And of course, it would be closer to Jumblese to write these sentences as, say, ‘CAESAR’ and ‘Caesar’ than to include an element ‘dying’: for Sellars’s (putatively neo-Tractarian) idea was that predicating something of an object is most perspicuously represented not by the concatenation, with the name of the object, of a further symbol for the predicate but by a modification of the name. So it may seem (now that I point this out) that I was too hasty when I moved from the thought that the forms of the verb ‘to die’ indicate the presence of an inferential commonality to the thought that they recommend the introduction into our begriffsschrift of some *string*, say ‘dying’, to mark that commonality, rather than this sort of Jumblese-style typographical quirk. However, I think that the point to be made four paragraphs below in the text will discharge whatever potentially illegitimate hypothesis this move rested on. (In any case, we shall discuss Jumblese in a little more detail in Chapter V.)

Now, it may be that *one* of these signs serves to convey nothing more than that the two elements represented by ‘Caesar’ and ‘dying’ are *to be combined* (since, after all, ‘Caesardying’ would be hard to read). Thus Frege himself uses parentheses to enclose the names for arguments of a function, and the comma to separate them when there are more than one. But it cannot be that *both* signs do this; if they both “conveyed nothing more than that the two elements were to be combined” (*tout court*), then we might as well represent both sentences, once again, as ‘Caesar—dying’, falsely to the differences in their inferential behaviour. Instead, one of the signs, at least, must contribute something further of its own. And if the pressures of *begriffsschriftliche* systematisation have in this way driven us to include among our symbols ‘/’ and ‘\’, then our earlier train of thought—according to which the *begriffsschrift* represents at once the categorial structure of thought—will lead us to suppose that (again, at least one of) these symbols represents a further element of thought.

But now note, in connection with our earlier remarks about the formal-semantic interpretation of the *begriffsschrift*, how apt this system is for a *functional* interpretation. Just as, at the linguistic level, ‘Caesar / dying’ can be viewed as the value of a function applied to the words ‘Caesar’ and ‘dying’ as arguments, and ‘Caesar \ dying’ as the value of a different function applied to the same arguments, so ‘\’ and ‘/’ themselves (or, again, at least one of them), which (according to that earlier train of thought) have now emerged as representing further elements of the thoughts expressed with their aid, must represent elements whose (ordered) combination with the elements represented by ‘Caesar’ and ‘dying’ yields a unique thought. (Unique for the same reason for which we couldn’t have represented both sentences in the *begriffsschrift* as, say,

‘Caesar—dying’, as discussed above.) But then, again, there is no bar to our conceiving of the sort of combination at issue at the semantic level as *functional*.²²

Thus, for instance, we might understand ‘/’ as it appears in our rendering of ‘Caesar is dying’ as mere combination (and think of “mere combination” as expressing *present-tense* combination), and ‘\’ as an operator on sentences shifting them into the past; this would yield a tense-logical approach to the difference between the two sentences. Alternatively, we might suppose both ‘/’ and ‘\’ to indicate, implicitly, the *moment* at which (as we might in this case try to put it) the combination of Caesar and dying obtains, something along the lines of standard quantificational semantics for tense logic; we might then be encouraged to insert into our formal rendering of such sentences an explicit argument place for moments in place of the crude ‘/’ and ‘\’: thus e.g. ‘Caesar—_t dying’ (which we would introduce into our begriffsschrift together with quantifiers and the sort of apparatus necessary for expressing indexical reference to moments such as the present). Either way, the result would be a pair of sentences each of which is intelligible as a functional combination of its elements.

(What about the case where we represent the difference between ‘Caesar is dying’ and ‘Caesar was dying’ not with distinct *symbols* such as ‘/’ and ‘\’ but rather with a typographical distinction such as the superscript/subscript distinction?²³ First of all, I hope it is clear that in the present case, the two are mere notational variants, so that what follows about the interpretation of the system with ‘/’ and ‘\’ will surely hold too of the system with the typographically represented

²² Just *what* function is at issue in a given case is not to be supposed to be too easily read off from the formalism: in $\forall xFx$, ‘ $\forall x$ ’ does not denote a *truth* function of Fx , for example, and likewise for e.g. \Diamond and \Box in modal logics. But the meaning—in some sense—of $\forall xFx$ is a function of the meaning of Fx : just what sense of ‘meaning’ is at issue is to be spelled out by the semantic apparatus; but on whatever parameters the semantic value of the compound ends up depending, that dependence will be construable functionally.

²³ And *ditto* for the even more Jumblese approach of writing ‘CAESAR’ and ‘Caesar’ rather than introducing a symbol ‘dying’: see note 21 above.

distinction. And in general, there would only be an obstacle to our recasting a notational system making use of such typographical distinctions to represent differences in modes of combination of elements as a system representing those differences using further symbols, if the typographical distinctions in question were as it were *continuous*: if they were used to represent a range of distinctions so fine-grained that even, for instance, a range of marks with numerical subscripts that could take any rational value would not suffice to represent the same distinction. (Thus imagine that, again Jumblese-like, we say that an object has a certain colour by writing its name in that colour.) But it should be clear that, whatever the interests of such a system might be, it would no longer be a *begriffsschrift* in our (that is, Frege's) sense; for inference rules making substantive use of such a typographical phenomenon would not be characterisable in the sort of recursion on the grammar constitutive of a formal system.²⁴)

At this point we may retract, if we wish, our hypothesis that 'Caesar is dying' and 'Caesar was dying' share not one but two common elements (though, for the reasons adduced earlier, it is an awfully plausible hypothesis). The moral at which we have arrived—namely, that, when we *do* find ourselves treating distinct judgments as composed, in our *begriffsschrift*, of common elements, we must find as well a further element in at least one of the judgments, construable in functional terms, representing (what we might otherwise have wished to call!) the mode of combination in question—is one which will hold for any such case, whichever cases those turn out to be.²⁵

²⁴ This point will become crucial—and will be expounded with a little more care—in chapter V, in our consideration of the development from Wittgenstein's *Tractatus* to his paper "Some remarks on logical form" and beyond.

²⁵ This "moral" is, I take it, expressed in the penultimate paragraph of (the version I have seen of) the introduction to Thompson's forthcoming book. However, it is there simply asserted; I have here attempted to argue for it. (A quibble: Thompson puts the point in this way: The idea that some signs might be "syncategorematic" in the classical sense is "unknown in Frege's actual system of representation, in which every elementary sign purports to have a 'reference'" (forthcoming: Introduction). But as we have seen, Frege uses parentheses and commas, which have no reference. Perhaps Thompson intends 'elementary sign' in a way that excludes the parentheses—or, more likely, he

§4. Predication as functional.

We have seen that Frege's conception of a *begriffsschrift*—and in general, the idea of a formal system—brings with it (at least as a guaranteed-to-be-viable option) the conception of the thought-elements denoted by the grammatical elements of the system as functionally composed.²⁶ In particular, for instance, we arrive at Frege's own theory of predication. In “Function and concept,” Frege announces that he will broaden the notion of a function (as it was used in his time in mathematics) from what are in effect operations (or constructions out of operations) on numbers²⁷ to something closer to the modern conception of a function, namely, any mapping from elements of one set to those of another, without restriction on the nature of the elements.²⁸ In particular, he proposes to supplement the battery of operations already recognised from which functions may be constructed, such as + and -, with the signs =, < and > (which do not, it is important to see, stand for operations), so that in addition to functions such as ‘ $\zeta + 2$ ’,

is placing a lot of weight on ‘in the classical sense’. In any case, I think the point is put more perspicuously by saying that in Frege all combination is *functional*, and so there is no need for the syncategorematic except in so far as it helps to make clear the functional structure (as parentheses do). Again, Thompson has (something close to) the point about functional combination too; I take myself not to be making a wholly new point, but to be trying to give an account of it.)

²⁶I do not of course claim that everyone who so much as puts forth a formal system at once presents a “conception of the thought-elements denoted by the grammatical elements of the system,” whether as functionally composed or otherwise: a notable (indeed obvious) counterexample is Hilbert's doctrine of formalism. The qualification “at least as a guaranteed-to-be-viable option” is meant to accommodate this.

²⁷Perhaps ‘operation’ (or even ‘construction out of operations’) does not cover the case Frege describes, as already extant in mathematical practice, “of a function whose value is 1 for rational and 0 for irrational numbers” (“Function and concept,” p. 12); but in any case what I want is just that this function's domain and range, like those of the others he canvasses, consist only of numbers. Still, it's interesting that this function is awfully close to what Frege's own treatment of the concept ‘ ζ is a rational number’ would be (differing only in that i) it's defined only for numbers as arguments, and ii) its range is {1, 0} instead of {the True, the False}).

²⁸Actually, Frege's is not *quite* the modern conception of a(n extensional) function, according to which, after all, any set of ordered pairs can be taken as a model of a function. In contrast, Frege's remarks about functions would seem to exclude, for instance, one presented by {<2, the True>, < ζ is red, the Sun>, <&, $\zeta \leq 3$ >}, for two reasons: both because of his “requirement for functions in general that they must have a value for every argument” (1891: 20), and because Frege insists, in addition, that “functions whose arguments are and must be functions are fundamentally different from functions whose arguments are objects and cannot be anything else” (1891: 26-27). (Indeed, this second reason tells us something about how we are to understand the first one: when Frege speaks of “a value for every argument,” we are to hear that generality as restricted to a particular logical type.) We shall be reminded of this characteristic of Frege's view of functions as in this sense *typed* when we come to consider his treatment of quantification, as contrasted with Wittgenstein's in the *Tractatus*, in Chapter V.

we have ones such as ‘ $\zeta = 2$ ’.²⁹ As ‘ $\zeta + 2$ ’ yields a number as value for any number taken as argument, Frege must explain what values ‘ $\zeta = 2$ ’ yields. As he puts it: “I now say, ‘the value of our function is a truth-value’, and distinguish between the truth-values of what is true and what is false” (“Function and concept,” p. 13). That is to say, he introduces truth values as possible values (and also arguments) of functions. And he goes on to propose to treat (first-level) predicates as referring to functions taking objects generally as arguments and mapping them into the set containing these two new objects.

This is a rehearsal of a familiar theme; my purpose in presenting it is to bring out how congenial this conception of the semantic value of predicates is to the conception of a *begriffsschrift* we have been examining. Conceptualising predication as functional in this way meshes perfectly with the idea of a *begriffsschrift*: for a function just *is* something which, fed a given argument, yields a given value: the notion as it were embodies the idea, emerging from our discussion in §3, that the referents of the symbols of a given *begriffsschrift* proposition combine in only one way.³⁰

I should emphasise that, as will emerge more clearly in the next section, Frege’s decision to treat *predicates*, in particular, as standing for functions *into the set of truth values* is simply one way of precisifying the *general* idea of a semantics based on the idea of functional application. It is that general idea, and not merely Frege’s particular way of spelling it out, that I am saying is

²⁹ Anscombe (1987-1988) highlights the momentousness of this move.

³⁰ By the way, note that the claim is not that, since (in a linear notational system) there is only one form of combination, *viz.*, concatenation, there can only be one form of combination at the semantic level. On the contrary, Frege is at pains to point out the essential difference between, *e.g.*, the relation between a concept and an object that falls under it, on the one hand, and the relation between a second-level concept and the concept that “falls within” it, on the other (“On concept and object,” p. 201)—both of which relations, one might say, are represented in the *begriffsschrift* by concatenation. (I am abstracting from the use of parentheses.) But where the mode of combination is, as here, distinct, this is indicated, even if not by a difference in the syntactic expression of combination, yet by a difference in the expressions for the combinants: for the notation is typed. What is essential is that, *given* a string of *begriffsschrift* symbols (and, hence, given their types), what concatenation represents in their case is settled. Thus combination is in every case functional.

congenial to the notion, under discussion in this chapter so far, of a begriffsschrift. Frege himself sometimes articulates the very general idea: for instance in “Compound thoughts” (1923-26: 37): “... it is natural to suppose that, for logic in general, combination into a whole always comes about by the saturation of something unsaturated.”

(To forestall a misunderstanding, perhaps I ought to add that when I claim that Frege is here presenting a “theory of predication”, I do not mean anything which would stand in tension with Frege’s own disavowal of the traditional approach to logic resting on the surface-grammatical distinction between subject and predicate. Granted, it would be a violent misreading of Frege to take it that his innovative application of function-argument analysis to logic consisted in his taking all of and only what had hitherto been regarded as predicates to be functions, and all of and only what had hitherto been regarded as subjects to be arguments. For one thing, one of the cardinal differences between Frege’s and the traditional approach is that function-argument analysis allows, in general, *multiple analyses* of a given proposition, as Frege repeatedly highlights: in ‘ $2^2 = 4$ ’, we may think of the function now as ‘ $\zeta^2 = 4$ ’, now as ‘ $2^\zeta = 4$ ’, ‘ $2^2 = \zeta$ ’, or ‘ $\zeta^\zeta = 4$ ’, and so on (and of what fills ‘ ζ ’ in each case as argument). For another, a fundamental insight of his into the nature of quantification is that quantifier phrases must not be run together with proper names, as the traditional subject-predicate distinction may tempt us to do.

Nevertheless, we can see that we have in Frege’s semantics a “theory of predication”, indeed a *functional* theory of predication, if we consider first, sloganistically, that he does after all, as we observed, understand *predicates* to refer to functions—specifically, to functions into $\{T, F\}$, which he calls concepts; and secondly, less sloganistically (and more generally), on whatever parsing of a judgment you like, such as one where the argument is a concept and the function a second-level concept, still the *mode of combination* of the elements of the judgment is

understood in functional terms (and indeed, on Frege's specific conception of this idea, the function in question will always be such as to map its argument onto an element in $\{T, F\}$). In short, we may say that by "a theory of predication" we mean something like "a theory of the unity of the thought," such that both the traditional subject-predicate analysis and Frege's function-argument analysis constitute theories of predication—theories of the constitution of thoughts and of how their elements hang together. It is in this sense that I say that Frege's "theory of predication" is functional.)

§5. Suspicion of the functional theory of predication.

Now, it is worth noticing, first, that Frege's introduction (by fiat) of nonoperations such as $=$, $<$ and $>$ to the set of building blocks from which functions may be constructed leads him immediately to have to countenance as grammatical propositions, indeed in some cases as expressions of truths, strings which would hitherto have been taken for gibberish, such as ' $(2^2 = 4) = (2 > 1)$ '. I do not mean to imply that this practice is unintelligible or incoherent, but simply to highlight the revisionism it involves.

This revisionism becomes more acute (even if still soft-pedaled) when, after a handful of arithmetical examples, Frege writes:

If the value of the function $x^2 = 1$ for an argument, *e.g.* for 2, is the False, we can express this as follows: ... '2 does not fall under the concept: square root of 1'. We thus see how closely that which is called a concept in logic is connected with what we call a function. Indeed, we may say at once: a concept is a function whose value is always a truth-value. ("Function and concept," 15)

Now, we may be inclined to "say this at once" if the only examples we have before us are mathematical. But remember: a function is a *one- or many-one* relation: that is, it is something which, when applied to a given argument (or, in general, to a given n-tuple of arguments), consistently yields the same one value. Again, this *does* jibe well with the concepts used in

mathematical cases. But there is in fact a great range of concepts—the vast majority of them, one wants to say—which don't, *prima facie*, appear to be modeled quite this well by the notion of a function. Any concept whose predication of the same object(s) is now true, now false would seem, again *prima facie*, ill-suited for modeling by a one- or many-one relation: thus 'ζ is pleased', 'ζ is cloudy', 'ζ is grateful to ξ', and so on. The only predication of a nonmathematical concept Frege invites us to consider over the course of the entire lecture (namely, 'Caesar conquered Gaul') is one which has obtained for some two millennia, so it does not invite immediate reflection on its temporality—but the point remains that it is obscurantist to declare that "we may say at once" that concepts are functions without even a nod in the direction of the great many concepts for which the functional theory appears at first glance quite hopeless.

§6. Response to the suspicion.

Of course, the previous section articulates a naïve view; it will be obvious how the "functionalist" will respond. Indeed, we have provided the materials for a response already: for we pointed out at the beginning of §3 above that mere intuition does not constitute sufficient grounds for finding a given compositional structure in a judgment; and we pointed out in the penultimate paragraph of §4 that Frege's is just one way of giving a functional model of the semantics of predicates. In particular, in this case, if we are pressed to fit our judgment into a function-argument mold—and the whole force of this chapter has been to show why Frege's overarching project, and the conception of a *Begriffsschrift* which serves as his means for carrying it out, *does* press him in this direction—then we can insist that, despite appearances, the relevant concept at issue in 'Caesar conquered Gaul' is not 'ζ conquered ξ' but, for instance, 'ζ conquers ξ at τ'. (Or we might consider finding in the judgment a predicate 'ζ conquers ξ' where what fills the place of ζ is not the name of an individual but the name of a "time-slice" of an

individual. Or again, we might consider ‘Caesar conquered Gaul’ *itself* a function from times to truth values. In this case, too, we preserve the claim that ‘ ζ conquered ξ ’ is a functional element in the judgment, though notice that in this case it no longer denotes a concept in Frege’s sense, but rather a function from pairs of objects to functions from times to truth values³¹. And such an approach can be generalised, to yield a conception of meanings of sentences as (still functionally) dependent on a wide range of contextual factors for completion.) Indeed, if for any of various reasons we find ourselves dissatisfied with Frege’s idea that the *Bedeutung* of a thought is a truth value, we may stray even further from his specific approach to predication—while all the while holding firm to the notion that propositional unity consists in functional combination—by understanding a predicate as referring to a function from the set of possible worlds into the set of objects, or by thinking in terms of situations instead of possible worlds, for instance.

Such a response to the objection that predication is not in fact perspicuously modeled as functional—namely, the proffering of further, tacit argument places, and further types of possible argument, by means of which some element in any given judgment can be made out to stand for a function after all—can seem obligatory, again precisely because of the pressure that the idea of a *Begriffsschrift* puts on us to find function-argument structure in the judgments we regiment, combined with the idea that our judgments must indeed be *susceptible* of such regimentation. A rejoinder to the response, demonstrating that this is not always possible, will have to be long and

³¹ as we would put it; Frege would not speak of “functions from times to truth values,” since a function for him must be defined over everything in the logical category of its inputs. Thus if we had been persuaded to introduce “times” into our ontology, as this approach presupposes, then that is to say that (as Frege would see it) we would have come to consider times “objects”, and we would thus consider not “functions from times to truth values” but functions from *objects* to truth values—or indeed, functions from objects to *objects*, which latter happen always to be truth values—that is, first-level concepts. (Cf. how quickly Frege infers that his newly discovered “truth values” are objects.) But of course we capture the intended idea in Frege’s framework by stipulating, for instance, that such a function yield the False as value for any argument that is not a time.

detailed; the following chapter will attempt to provide one for the case of what Thompson calls “natural-historical judgments.” But let the dialectical situation be registered: Frege starts out with what Wittgenstein might have called a one-sided diet of examples, on the basis of which (though of course not simply gratuitously, as we have seen) he offers a theory of predication which appears singularly unsatisfying for virtually any example of a concept besides those from the field in which he is primarily interested. The defender of the theory attempts to fit the apparent counterexamples—that is, again, the bulk of what would ordinarily be considered concepts—into the Procrustean bed, by searching for hidden arguments in the judgments at issue. If she is successful, the rewards are rich: we can assimilate more and more swaths of discourse to the regimentations of the *begriffsschrift*. But it is worth enquiring into the question whether the conviction that this *must* be possible isn’t after all ill founded.

Chapter Three. The semantics of “natural-historical judgments”.

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§1. Introduction: Thompson on “natural-historical judgments.”

In “The representation of life” (1995), Michael Thompson undertakes an investigation of the expression of what he calls “natural-historical judgments,” that is, judgments describing the natural history of a kind of living thing. He claims that sentences expressing such judgments—paradigmatically, sentences with the form “The S is F”, which he calls “Aristotelian categoricals”, such as ‘The bobcat breeds in spring’—exhibit a distinct *logical* form: distinct from that of any other kind of sentence, and in particular not resolvable into the kind of quantificational forms familiar to logicians since the work of Frege. He claims, however, that his

investigation is nevertheless in the spirit of Frege: for, just as Frege taught us to distinguish the *logical* categories *object* and *concept*, so Thompson is drawing our attention to the *logical* categories *life form*, *vital operation* and so on. Thompson writes: “It is one of the lessons taught by Ludwig Wittgenstein, if I understand him, that we must recognize many intuitively more determinate distinctions *of the sort Frege introduced*” (1995: 249n4, his emphasis).

This claim will surely be met with raised eyebrows by most logicians. There is of course a range of opinions as to the proper demarcation of logic; but the criterion by which Thompson singles out his favoured class of judgments seems so palpably to be a matter of *content* that the attribution to them of a unique logical form will just seem to rest on a confusion. In particular, I suspect that most logicians will only see promise, if in anything, in the attribution of a unique logical form to *generic sentences generally*. That is, they may be willing to be persuaded that generic sentences such as ‘The bobcat breeds in spring’ or ‘Polar bears are at risk of extinction’ or ‘Canadians favour the social safety net’ can’t be recast faithfully in classical first-order predicate logic unsupplemented; but however they *are* to be understood, they are all to be understood in the same way—or, if divisions are to be made, they should be made along more obviously *logically* salient grounds, such (perhaps) as the syntactic distinction between the (apparent) singular predication of the first example as contrasted with the indefinite plurals in the second two¹, or (perhaps) the semantic distinction between predicates that seem to hold only of *kinds* (such as that in the second example) and predicates that hold of a kind’s instances (such as those of the first and third examples)². But the isolation of a subset of generics on the grounds

¹ Thus Ariel Cohen (2001), for instance, argues that the logical behaviour of generic sentences with bare plural noun phrases in subject position differs systematically from that of those with singular descriptions—or, more precisely, that the former class of generics exhibits an ambiguity not seen in the latter. (We shall examine his work in a little more detail below.)

² Though they do not put the point in quite the same way as I have here, this is in effect the same distinction as that drawn by Gerstner and Krifka, who call sentences containing the former “D-generics” and those containing the latter

that their subject is a *form of life* will seem gratuitous to them. (This is so partly because it will cut across the boundaries that will appear more natural: thus the sentence about bobcats could, it would seem, be indifferently expressed in the indefinite plural: ‘Bobcats breed in spring’.³)

Thompson anticipates something like this train of thought, and has this to say in response (I shall quote at length):

Perhaps then our sort of proposition [*viz.*, expressions of natural-historical judgments] should be brought under the linguists’ rubric *generic sentence*, and we should follow them in their attempt to supply a ‘semantic’ analysis. Here, though, we meet with a different sort of problem. It is not that the suggestion is simply false. If the class of generic sentences is marked off by possession of some such outward form as the unquantified ‘bare’ plural ‘*Ss are F*’, then there is no question that a natural historical judgement *can* be expressed in a ‘generic’ sentence. But is there any reason to think that the class of generic sentences, so understood, is not a rag-bag covering many forms of conjunction of subject and predicate—our own type just one among them? ... [A] similarly identified class of ‘statements with a definite description as subject’ would have to constitute a merely surface-grammatical category: it is clear that the words ‘The domestic cat has four legs’ contain a syntactical ambiguity, and that the natural reading is not the one Russell attempted to explain.

It is implicit in Aristotle’s remarks that inferences involving judgements *hōs epi to polu* should mirror those involving universal judgements. And it does seem true that, just as ‘All *As* are *F*’ and ‘All *As* are *G*’ together entail ‘All *As* are both *F* and *G*’, so also ‘The *S* is *F*’ (or ‘*Ss* are *F*’) and ‘The *S* is *G*’ (or ‘*Ss* are *G*’) together entail ‘The *S* is both *F* and *G*’ (or ‘*Ss* are both *F* and *G*’)—if it is our sort of combination that is expressed. The inference would obviously be invalid for any sort of statistical generalization. And it would be too bold to claim that it holds for generic statements or bare plurals generally, if only because the bare plural can presumably express a form of statistical generalization... A typical page of biochemical exposition exhibits none of the inferential anxiety that would be called for if the propositions it contains expressed mere statistical generalizations or if they were to admit only the inferences that we can suppose hold generally among what linguists call generic propositions. (1995: 285-286)

With these paragraphs Thompson disposes, as irrelevant to his enquiry, of all investigations of generic sentences carried out by linguists and formal semanticists.⁴ But again, I suspect that most logicians would remain unpersuaded. First—though there is surely *something* to the charge

“I-generics”. Krifka *et al.* (1995), crediting Gerstner and Krifka, distinguish likewise between “sentences with kind-referring NPs” and “characterizing sentences” (with the qualification that some sentences can exhibit both phenomena). And Asher and Morreau (1995), for instance, explicitly confine their remarks to the latter class.

³ (‘It would seem’ because, as mentioned in note 1 above, not all researchers agree that these two forms of expression are synonymous.)

⁴ Of course, Thompson has plenty of *other* sorts of argument for his claim that natural-historical judgments are of a unique logical form (some of which will be considered elsewhere in this chapter); it is perhaps uncharitable to present these two paragraphs, out of context, as Thompson’s only grounds for refusing to treat them together with other generic sentences. But I hope it will be agreed that the uncharitable presentation serves an innocuous purpose, revealed just below in the text: to urge more careful consideration of the proposals in the literature for dealing with generics, along the lines of the careful consideration Thompson gives to other approaches to natural-historical judgments alternative to his own.

(expressed in the first quoted paragraph) that the category *generic sentence* applies to a “rag-bag” of logically distinct types of sentence—a glance at the literature will show that Thompson’s charge is too quick as it stands. For the investigators of generics *don’t* (or at any rate *don’t all*) mark off their subject matter “by possession of some such outward form as the unquantified ‘bare’ plural”; on the contrary, most of them recognise both that the sentences that interest them come in different surface-grammatical forms (notably, the two forms Thompson himself refers to in this passage, rendered schematically as ‘The S is F’ and ‘Ss are F’, to which we might add a third, ‘An S is F’) and that some sentences that *don’t* interest them—that aren’t generic—share those surface forms with those that do. Further, even when scholars set out to discuss the “rag-bag” of generics as a whole (as distinguished, on whatever grounds, from their syntactically similar counterparts), their first step is frequently to note, precisely, Thompson’s point, that the class is in one sense or another heterogeneous: thus (as we observed in footnote 2 above) Krifka *et al.* distinguish between “sentences with kind-referring NPs” and “characterizing sentences”; Koslicki (1999) gives a “two-tiered” account on which the plural predication she finds in all generic sentences gets different semantic treatment in different cases; Cohen (2001) claims that generics with bare plurals are systematically ambiguous in a way that generics with singular descriptions are not; and so on. If investigators of the semantics of generic sentences recognise as well as Thompson does that they do not form a homogenous class, then the fact that they do not form a homogenous class would not appear to demonstrate that their results can be of no value for Thompson’s own investigation. If they do not end up finding grounds for isolating a subclass as narrow as Thompson wants, it is surely worth at least looking into the question whether their coarser-grained analysis isn’t after all sufficient to capture all the *logically* significant differences among the sentences they investigate.

The point Thompson articulates in his second paragraph is subtler: for the discovery that a class of sentences can be subdivided into subclasses exhibiting systematically different *inferential* behaviour, as Thompson claims is so for the class of generic sentences, will certainly constitute strong *prima facie* grounds perfectly intelligible to a logician for considering the initial class logically heterogeneous. The trouble in this case is that since, again, investigators of generics don't isolate their subject matter on a surface-grammatical basis, they are not committed to lumping statistical generalisations together with, for instance, natural-historical judgments; on the contrary, they frequently open their discussion by *distinguishing* between generics and mere statistical generalisations.⁵ What Thompson observes about the inferential patterns supported by natural-historical judgments does appear to give good grounds for distinguishing them, logically, from statistical generalisations; but, again, if those working on generics have observed the same thing, we do not yet have a reason for dismissing their findings as having missed the mark Thompson has in his sights. Again, if they can find a *broader* class of "generics" than Thompson's natural-historical judgments but including them (and excluding, if Thompson's argument is correct, statistical generalisations), whose inferential behaviour appears homogeneous, they will cast doubt on Thompson's claim for the uniqueness of the logical form of natural-historical judgments.

In short, at least some of the work that has been done on generic sentences is sensitive to the worries Thompson articulates, and yet persists in treating them, even if not as a merely surface-grammatically delineated class, still as a class quite a lot broader than Thompson's class of natural-historical judgments. For this reason, I think there is some value, after all, in canvassing

⁵ Even Cohen in his "Generics, frequency adverbs, and probability" (1999), who comes closer than anyone else discussed in this chapter to assimilating generic sentences to statistical generalisations, presumes as obvious the distinction between generics and frequency statements.

the proposals that have been mounted by linguists and formal semanticists, to check with greater care whether one of them doesn't after all succeed in capturing the "logical form" of natural-historical judgments. If one did, it would vitiate Thompson's claim that their form is unique. — As a matter of fact, I shall argue that he is right after all—but (lest the reader here feel as though she is about to be asked to waste her time) it will emerge that part of what is at stake in this investigation is the *very concept of logical form*; and in particular, I shall argue that Thompson was wrong to claim that his conception of logical form is just the same as Frege's, only enriched by a broader range of categories.

Approaches to generic sentences fall, for the most part, into two types: they either treat them as expressing a special kind of distribution of properties over individuals, on some level analogous to quantification, or they treat them as predicating something not of individuals (even in some sort of specially distributive way) but of a(n in some sense singular) kind. There are variations not falling solidly into either of these camps: for example, "hybrid" approaches, dividing the class of generic sentences up in one or another way and proposing a distributive reading of some and a kind-predicative reading of others. But if we divide our own discussion into two sections, the first treating of distributive and the second treating of kind-predicative approaches, we can consider along the way the hybrid approaches, by discussing each "half" of such an approach in the appropriate section. —Since each approach conceives of and/or divides up "the class of generic sentences" somewhat differently—some on syntactic, others on semantic grounds (as should be clear from the above discussion)—we'll have to keep track of these differences; but all the while we'll keep in mind that our main interest is in the implications each proposal has for the analysis of Aristotelian categoricals and other expressions of natural-historical judgments.

§2. “Distributive” approaches to generic sentences.

A good reason to take generic sentences as subject-predicate in structure—and to take the subject in question to denote a *kind*—is that, in Gupta and Savion’s words, “the only plausible alternative..., that of taking generic sentences to be implicitly quantificational, can survive refutation only by being evasive on the nature of quantification involved” (1985: 859).

However, since some scholars advocating such “quantificational” approaches to generics are less evasive than others—for one instance, Krifka *et al.* spend twenty pages on the question of the semantic treatment of a quantifier-like generic “operator”, canvassing six different approaches—it is worth at least considering whether any of them after all succeeds.

For the sake of establishing some continuity in the discussion, let us use the following notation, borrowed and simplified from Krifka *et al.* and inspired originally by Lewis’s treatment of “adverbs of quantification” (1975), as a framework with reference to which to expound the various “distributive” approaches we shall be considering⁶:

$$\mathbf{GEN}[x; y](\mathbf{Restrictor} [x]; \mathbf{Matrix} [\{x\}; y])$$

Here, **GEN** is the generic “quantifier”; x is the variable it binds; y is bound existentially, and appears only in the matrix; the **Restrictor** expresses the “subject concept” of the generic sentence, as we might put it; and the **Matrix** expresses its “predicate concept”. The curly braces

⁶ As mentioned earlier, Krifka *et al.* (1995) distinguish between “characterizing generics” and sentences with “kind-referring NPs”, where the former, such as ‘Jaguars hunt at night’, appear to distribute their predicate, in one way or another, over individuals, while the latter, such as ‘The dodo is extinct’, seem to predicate something directly of a kind (*e.g.*, it’s not that individual dodos are extinct). They introduce the notation discussed in the text in the course of their discussion of characterising generics, and limit its application to them. But note that the intuitive distinction they draw between characterising generics and sentences with kind-referring NPs is optional, in the sense that it is open to a theoretician to insist that, despite appearances, all generics are to be analysed in the same way (or, at any rate, to deny that the intuitive distinction “cuts at the joints”). I shall therefore treat “distributive” approaches as approaches to the semantics of generic sentences generally—as many of their proponents intend—rather than as approaches to characterising generics in particular.

indicate that x may or may not appear free in the matrix.⁷ We shall make frequent use of the vocabulary of restrictor and matrix in our discussion below. Where an author’s semantic proposal rests on an essentially different initial formalisation, we shall indicate as much.

§2.1. Bacon’s proposal.

John Bacon (1973) raises the question whether “generic descriptions” denote. By “generic descriptions” he means expressions appearing in generic sentences which are syntactically similar to definite descriptions, but which appear to carry generic force: for instance, ‘the lion’ as it appears in the generic sentence ‘The lion is tawny’. By the question whether they denote, Bacon means to ask whether the semantics of such expressions should be understood as analogous to that of proper names. He argues, first, that they are grammatically similar not only to definite descriptions but also to indefinite singular noun phrases (such as ‘a lion’), plurals, mass nouns, and “abstract” singular terms, and unlike proper names. He suggests that, on the basis of this grammatical similarity, they should be treated as semantically analogous to them as well. And Bacon next claims, crediting Sellars, that it is a necessary condition of an expression’s being a proper name that it not exhibit scope ambiguities, while all these types of expression, generic descriptions included, exhibit such ambiguities, for instance in the ways they interact with negation. (Thus, the suggestion goes, ‘The lion is not tawny’ can be understood as meaning that it’s not the case that the lion is tawny—making no positive generalisation about their colour—or else as meaning that the lion is not-tawny, i.e. that lions, generally, positively fail to be tawny.) In short, they do not denote (in the sense in which proper names denote). Instead,

⁷ The more general notation used by Krifka *et al.*, to accommodate the possibility of many-place relational subjects and predicates, is **GEN**[$x_1, \dots, x_i; y_1, \dots, y_j$](**Restrictor** [x_1, \dots, x_i]; **Matrix** [$\{x_1\}, \dots, \{x_i\}; y_1, \dots, y_j$]). Thus for instance ‘Jaguars give birth to two to four cubs’ would be rendered as ‘**GEN**[$x; y_1, \dots, y_4$](x is a jaguar and $y_1 \dots y_4$ are jaguar cubs and x gives birth to y_1 and ... and x gives birth to y_4 and $y_1 \neq y_2$)’.

Bacon suggests that they all be interpreted as quantificational.⁸ Generic sentences, in particular, should be understood as expressing either straightforwardly universal quantification or, “[o]ccasionally,” “normatively restricted” universal quantification.⁹ In explanation of this latter, Bacon quotes Frege approvingly: as Frege puts it, “‘the horse is a four-legged animal’ ... is probably best regarded as expressing a universal judgment, say ... ‘all properly constituted horses are four-legged animals’” (1892: 196). Though Bacon offers no further help with interpreting such restriction, he does point out that even explicitly universal quantification using ‘all’ or ‘every’ is in fact sometimes best understood as “hyperbolic”, and as intended to be restricted to some smaller domain (left implicit).

Now, the negative point Bacon draws from Sellars—that the kinds of scope ambiguity visible in generic sentences, even those whose subject term syntactically resembles a definite description, stand in tension with the thought that that subject term makes singular reference—is one that any account of generics as (say) singular predications of kinds must come to terms with, and not without difficulty.¹⁰ However, it should be clear that Bacon’s positive proposals do not come to much. The thought that generic sentences express straightforward universal quantification is palpably false; ‘The lion is tawny’ is not falsified by the occasional lion born snow white due to a recessive gene.¹¹ The Fregean suggestion that some sort of *restriction* on the quantification is in play is a little more helpful; but, though the observation that even nongeneric universal

⁸ In fact he insists that the analogy between generic and definite descriptions is tight indeed: a generic description, he holds, is just like an ordinary definite description, but used in a situation in which the context alluded to is understood to be the universe of discourse as a whole, so that the sentence is true just in case everything satisfying the description is true—in other words, it is a universal quantification. (I am ignoring the details of his account of the relations between singular and plural.)

⁹ I’m setting aside cases where, as Bacon points out, an *existential* reading seems more appropriate: e.g. (his examples), ‘Chryslers are sold on the West Coast’, as opposed to ‘Chryslers are sold in North America’.

¹⁰ Gupta and Savion (1985) made the same observation, in the review cited earlier.

¹¹ This point is strongly emphasised by Thompson; but further, their admission of exceptions is also the first characteristic of generics observed by almost all scholars working on them from linguistic and formal-semantic perspectives. In this respect Bacon is unusual in his flat-footedness.

quantification is often “hyperbolic” is one we shall see again (in a more refined form)¹², failing any *account* of the kind of restriction on quantification at issue, we are still without an understanding of the semantics of the sentences in which we’re interested. Of course, we saw that Bacon does *gesture* in the direction of an account: he refers us to Frege’s thought that the restriction in play is “normative,” and the quotation from Frege suggests (though Bacon does not draw particular attention to this) that the restriction is to be interpreted as a restriction on the concept appearing in the antecedent of the conditional that is the object of quantification. But so far, this is just a gesture: we are told nothing about the semantics of the “normative restriction” itself, and indeed even a crucial syntactic distinction is left unspecified by the proposal: should we understand the ‘properly constituted horses’ of Frege’s example as of the form ‘ $P(x) \ \& \ H(x)$ ’ or as of the form ‘ $(P(H))(x)$ ’—that is, is ‘properly constituted’ a concept, or an operator on concepts (or indeed perhaps something else again)? We shall consider below two further approaches to generics filling out these two possibilities—respectively, “relevant quantification” and the “prototype” approach. But Bacon, at any rate, does not yet give us a full positive account of the semantics of generic sentences.

There is one worry we do well to voice already, however, concerning any account of generic sentences having anything like the shape Bacon gestures at. Keeping in mind that our particular goal in this enquiry is an understanding of sentences expressing what Thompson called natural-historical judgments, we may ask whether any account involving universality, however “restricted”, has a chance of capturing the force of such judgments. I can put my thought here a little paradoxically: though natural-historical judgments admit of exceptions (so that the naïve

¹² As we’ll see in §2.11 below, Asher and Morreau (1995) appeal to this idea in defense of a (broadly) quantificational interpretation of generics in the face of an objection of Carlson’s (1977); however, both Carlson’s objection and Asher and Morreau’s reply are more nuanced than anything here in Bacon.

rendering of them as straightforward universal quantifications is evidently hopeless), there is a sense in which they *are* universal, and it is precisely *that* universality which is lost in any attempt to *restrict* their universality. What I mean is this: any explanation of the semantics of, say, ‘The lion is tawny’ that has it ascribing tawny¹³ to (all members of) some subset of lions, or to some other population related in some way to the set of lions, misses the respect in which such a sentence has implications also for the lions that *don’t* fall in the favoured population. If I am told that lions are tawny, I’m told something that, in *some* sense, has application also to *this creature here*, even if this creature here is a white lion. (Contrast, for instance, ‘All small lions are tawny’: if we analyse *small* here as an operator on predicates (in order to capture its “attributive” quality), we might render the sentence as something like ‘ $\forall x[(S(L))(x) \rightarrow T(x)]$ ’. This looks to be of a form precisely analogous to the second interpretation of Frege’s suggested proposed earlier, but it’s clear that such a sentence as this simply has no implications for the lions that aren’t small.)

Let me try to make this rather airy thought a little more vivid. Consider the whole natural history of some species, consisting in a battery of natural-historical judgments. Interpreted along the lines of the present proposal—according to which each natural-historical judgment is captured by a universal quantification restricted to some subset of the species—such a history seems to be reduced to an assortment of characteristics associated with different groups of organisms: “*These* members of this species do *this*; *those* members are like *that*; *this other* group engages in *this* behaviour; etc.” Such a catalogue loses track of the *unity* that ties the whole natural history together: of that which makes it the history of a single species, that makes it—the whole history—in some sense *true of every member* of the species, even though (as Thompson points

¹³ (or, pleonastically, “tawniness”)

out) normally no single member will instantiate the matrices of all of the judgments. —Is this “unity” captured, in the model, by the fact that each subgroup is generated *from the species*—that is, by the fact that each individual natural-historical judgment in the history is a quantification over the set of members of the species, however it is further restricted? —But this is, formally, no different from, say, starting from the set consisting of the gorillas at the Pittsburgh zoo, the numbers one through nine, and the moons of Saturn, and producing a “natural history” of the members of that set by listing some universally generalised conditionals whose antecedents are restricted to its various subsets. It is clear that no such conditional restricted to a subset excluding Titan, say, tells us anything whatsoever about Titan or its “kind.”

Most scholars working on generic sentences start (as I observed in footnote 11 above) from the fact of their admission of *exceptions*. But the way this phenomenon is conceived in the sort of proposal now under discussion is to begin by interpreting the generality of generics quantificationally, and then to interpret the presence of exceptions as an indication that they don’t actually speak of the whole population of things satisfying their “restrictor”. However, what I’ve been trying to suggest is that the fact that they “admit of exceptions” *doesn’t* actually mean that they “don’t speak of” some of the things fitting the specification in the restrictor. Any attempt to formalise generics in a way so as to make room for exceptions simply by as it were *exempting* them (in one way or another) from the range of the generality is bound to be false to this fact I’m trying to articulate. Subject-predicate approaches look attractive precisely because they *don’t* work by simply leaving aside the members of the kind failing to satisfy the matrix: if a generic is just a predication of something directly of the (whole) *kind*, then there *is* a clear sense in which it’s “just as true of” any one (even a “defective”) instance of the kind as of any other instance. (But such approaches raise problems of their own, as we shall see in §3 below.)

§2.2. Fara's proposal.

Delia Graff Fara (2001) makes some remarks on the interpretation of generic sentences in the context of a broader campaign to reinterpret all kinds of descriptions—definite and indefinite, singular and plural, and including generic expressions—not as quantified noun phrases (as Russell did) but as complex predicates. She argues that such descriptions behave in ways disanalogous to the behaviour of quantifier phrases in certain types of situation: in particular, with reference to the phenomenon of scope ambiguity and to their interaction with adverbs of quantification. She urges that we can account for this behaviour by understanding descriptions as having predicate-like semantic values (which she supposes are sets), and she explains how such an account ought to go.

Fara begins by reminding us of Strawson's distinction between (what he called) "uniquely referring" and (what she calls) "descriptive" uses of definite descriptions: thus 'the greatest French soldier' has the former type of use in 'Washington met the greatest French soldier' and the latter in 'Washington was the greatest French soldier', for in the second sentence, Strawson held, the description is used simply to say something about Washington, while in the first, it is used to "mention an individual", as 'Washington' is. Fara argues that Russell's approach to descriptions is to take the "referential use" as paradigmatic: thus, when he explains how to analyse a sentence with a description, he always presupposes implicitly that the description is in "argument position" with respect to the surface-level grammatical context.¹⁴ She argues that

¹⁴ Fara's account of "surface-level argument structure" is a little puzzling. The standard conception of argument structure, if I am not mistaken, *contrasts* it with surface structure. Thus Frege introduced function-argument analysis into logic precisely by contrasting it with the subject-predicate distinction (see *e.g.* *Begriffsschrift* (1879: VII)). Frege also pointed out that until we have generality in view, the applicability of the notions of function and argument to the composition of a proposition is not uniquely determined: thus we can conceive of 'Socrates is wise' as the result of applying to the argument 'ζ is wise' the second-level function that returns the True as value for every first-level concept that is true of Socrates, and returns the False for all other arguments. Fara's decision to "leave

Russell wasn't very explicit about just how to apply his analysis to cases where the description has a "descriptive use," but she tries to show that a fully spelled-out account on Russellian principles would have to be awkward and *ad hoc* in places, in order to explain why descriptions in such uses frequently lack the sort of scope ambiguity Russell's theory would appear to predict.

Fara's approach, in contrast, is to take the "descriptive use" of descriptions as paradigmatic; she groups all descriptions together as "predicate nominals", and proposes to treat them consistently as having as semantic values sets whose members are the entities of which the predicate in question is true. Her task is then to explain how descriptions sometimes come to have "referential uses." Her claim is that when a surface-level analysis yields a mismatch of semantic types—when, that is, we find what ought to have a predicate-type semantic value itself in the argument place of a predicate—we deploy a rule of quantifier raising, transforming the surface-level argument structure into an argument structure at the level of Logical Form that respects the predicate-like semantic value of the description. In particular, she gives the following threefold "LF rule", for application to surface-level contexts $\Phi(PN)$ (that is, contexts in which a predicate nominal is, on the surface, the argument of another predicate):

$$\begin{array}{ll} (LF_1 PN) & \Phi(PN) \Rightarrow [\exists x : PN(x)](\Phi x) \\ (LF_2 PN) & \Phi(PN) \Rightarrow [Gen x : PN(x)](\Phi x) \\ (LF_3 PN) & \Phi(PN) \Rightarrow [Adv x : PN(x)](\Phi x)^{15} \end{array}$$

Again, one of the phenomena which, she argues, Russell's theory of descriptions ill accommodates is scope ambiguity. One of her examples is the contrast between, on the one

these notions [*viz.*, those of argument and predicate] at an intuitive level" (p. 3) thus seems disingenuous. Nevertheless, since these issues do not affect the substance of our discussion here (though they are crucial to her own concerns), I shall set them aside.

¹⁵ P. 26. I have renumbered Fara's subscripts. Perhaps the notation requires a little comment: each square-bracketed string is something like a restricted quantifier, where the predicate nominal acting as the restrictor is the description with which we begin: thus $(LF_1 PN)$ transforms 'The book is on the table' into $[\exists x : \text{the book}(x)](\text{on the table}(x))$, *i.e.*, 'There is something which is the book and which is on the table.'

hand, ‘A dog rarely eats vegetables’ and, on the other hand, ‘Most dogs rarely eat vegetables’ and ‘Fido rarely eats vegetables’. She argues that, while the second of these sentences, which is explicitly quantificational, exhibits—like the third—no scope ambiguity, the first is ambiguous among two or three readings: it could be read as making a generic statement about dogs (to the effect that they rarely eat vegetables); or as a claim that it is rare for a dog to be wont to eat vegetables; or even perhaps as the report that a certain dog rarely eats vegetables.¹⁶ (In general, a sentence with a description in surface-level argument place will be three-ways ambiguous when it also contains an adverb of quantification; if there is none, it will be two-ways ambiguous between the generic and the existential readings (which, Fara mentions in passing, could also be understood as involving unpronounced adverbs of quantification, yielding a slightly more unified account).) She argues that Russell’s theory, which analyses descriptions as quantificational phrases, can only explain this disanalogy between descriptions and explicitly quantified phrases in an *ad hoc* manner, while her account, with its threefold LF rule applied to predicate nominals (as which all descriptions are interpreted), handles it nicely.

I have left out much of the (rich) detail of her argument, because my interest here is in what she goes on to say about the semantics of generic sentences in particular. (Part of her advertisement for the theory she presents is that it “handles generic descriptions”, which Russell’s theory cannot do, and moreover that it does so in a way parallel to its treatment of other sorts of description: p. 2.) One might suppose that the upshot of her account for a generic sentence such as ‘The tiger is striped’ (her example) would be that it should first be parsed, at the surface level, as ‘Striped(the tiger)’, and then, since (on her proposal) ‘the tiger’ is to be understood as a predicate nominal having a predicate-like semantic value, it should be transformed in accordance

¹⁶ She concedes that the third reading here is unlikely, but suggests that it becomes recognisable when we consider the similar sentence ‘A dog I know rarely eats vegetables’.

with the rule ($LF_2 PN$) into ‘ $[Gen x : x \text{ is the tiger}](x \text{ is striped})$ ’. However, Fara introduces a complication: simple definite descriptions, she says, do not admit of generic or adverbial but only of existential readings, because a simple definite description can be true of at most one thing. (The contrast is with definite descriptions containing embedded noun phrases, such as ‘The owner of a Porsche’ (again her example), which can be used to make quantificational or generic statements precisely because there can in fact be more than one Porsche owner; the definiteness arises only from the fact that, *for a given Porsche*, there is (normally) only one owner.) Thus in fact the LF rule deployed in the semantic interpretation of ‘The tiger is striped’ must be not ($LF_2 PN$) but ($LF_1 PN$); the result is ‘ $[\exists x : x \text{ is the tiger}](x \text{ is striped})$ ’. And she recommends that we understand this existential quantification by giving the common noun ‘tiger’ what genericists have called its “taxonomic” reading: that is, the reading it has in ‘There are two tigers native to Myanmar: the Bengal and the Indochinese’. In short, Fara interprets ‘The tiger is striped’ to mean something like “There is a kind which is the tiger and which is striped.”¹⁷

Let us reflect on these results of Fara’s; and let’s begin by noting that her account, just discussed, of the semantics of ‘The tiger is striped’ differs quite dramatically from what must be her account of the similar ‘A tiger is striped’ (which she doesn’t discuss). Since the description ‘a tiger’ is indefinite, the epicycle she adds to her account in order to deal with simple definite descriptions does not apply, and her basic story of descriptions in surface-level argument position yields, for ‘A tiger is striped’, the usual ambiguity between the existential and generic readings. Since we are interested in the interpretation of the sentence as generic, we get the LF

¹⁷ This is actually quite close to what I have called a “subject-predicate” approach, to be discussed in §3 below. But since the overall structure of Fara’s account is plainly “distributive”, and since (as I shall discuss in the text immediately below) she offers no further details about the content of generics she gives this “taxonomic” reading, I am treating it here together with the rest of her account, rather than saving it for §3.

representation ‘[*Gen* $x : x$ is a tiger](x is striped)’. Here we have a generic quantifier, distributing the property of being striped generically across individual tigers. In contrast, her interpretation of ‘The tiger is striped’, as we saw, attributes the same property to the kind *tiger*. This should strike us as odd. I do not mean to say that it is an unassailable truth that generic sentences employing definite articles are synonymous with those employing indefinite descriptions.¹⁸ However, since most theorists of generic sentences do not treat them differently—and since, indeed, it seems after all quite natural to switch indifferently between the two, for instance in the narration of a nature documentary—Fara at least owes us some words in explanation of her surprising result that a generic in ‘The’ followed by a simple noun phrase can only be interpreted as a predication of a kind. However, she doesn’t seem even to notice the counterintuitiveness of her result.¹⁹

Moreover—and perhaps more gravely—Fara does not appear to notice the anomalous implications her account has for the semantics of such predicates as ‘is striped’. *Prima facie*, it seems as though ‘striped’, at least, appears univocally in ‘The tiger is striped’ and ‘A tiger is striped’ (even if the descriptions are to be interpreted differently). But as we have seen, in the former, on her analysis, the predicate is said to hold not of individual tigers but of the *kind*. But what can it mean to say that a species—not the individuals of the species, but the species itself—is striped? The obvious answer, I suppose, is that it is, after all, just a shorthand expression

¹⁸ An example of a similar contrast which is *argued* for can be found in Cohen (2001), whose account we alluded to above and shall discuss in more detail below: he argues that there is a systematic difference between generics expressed with indefinite descriptions and those expressed using bare plural noun phrases. Thus he would treat ‘Tigers are striped’ differently from ‘A tiger is striped’. We’ll examine the details below; the point here is that, unlike Fara, Cohen flags the disanalogy his theory implies and attempts to motivate its acceptance.

¹⁹ Could we appeal to Fara’s own use of the “taxonomic” reading to get the interpretations of these sentences back in line, by understanding ‘A tiger is striped’ as speaking (existentially) of one of the *kinds* of tiger? But such a reading of this sentence would yield, on her account, ‘[$\exists x : x$ is a tiger](x is striped)’—which is still not quite the same as ‘[$\exists x : x$ is the tiger](x is striped)’; and in any case, this existential taxonomic reading, though admittedly a possible reading, is clearly not the typical way to hear the generic ‘A tiger is striped’, the way according to which it is (at least *prima facie*) synonymous with ‘The tiger is striped’.

meaning that the individuals of the species are striped—though anyone who proposes to understand ‘The tiger is striped’ as, at the level of Logical Form, a predication of the species itself surely owes us an explanation of the mechanism by which this “shorthand” is to be spelled out (and at what level, if not at Logical Form?), and Fara gives us none. But in any case, what sort of generalisation is the statement that the individuals of the tiger species are striped? It is surely not a universal generalisation²⁰, but rather a *generic* claim. In other words, the genericity of ‘The tiger is striped’ is, far from being explained, in fact fully obscured by her analysis.

Next let us consider some of Fara’s remarks on the generic quantifier appearing in (LF₂ PN).

She writes:

A number of other semantic issues remain open. The most glaring of these is that while I have said what the semantic value of predicate nominals is to be, I have not assigned truth-conditions to all sentences containing them.... The generic quantifier I’ve helped myself to presents special problems.... Whether a sentence ‘[Gen $x : \Phi x](\Psi x)$ ’ is true is not just a matter of the number or proportion of things in the extension of Φ that are in the extension of Ψ . The generic quantifier is used to make *law-like* generalizations. Other well-known problems are presented by sentences like ‘Guppies give live birth’.... For these sentences to be true, it is not required that it be generally true of a guppy that it gives live birth..., but only that it be generally true of a guppy that gives birth that it gives live birth. (pp. 31-32, emphasis in original)

Fara adds in a note that the latter problem may be dealt with by invoking the idea of quantifier domain restriction, “in which case we need not view such cases as presenting a special problem for the interpretation of the generic quantifier” (p. 39n28, emphasis removed).

It is to Fara’s credit that she admits that she has nothing illuminating to say about the semantics of the generic quantifier appearing in her (LF₂ PN) rule, and hence in her analyses of most generic sentences. Indeed it is perhaps suggestive of the direction of our own enquiry that she remarks (actually about adverbs of quantification generally, but in a context in which it seems as

²⁰ As it happens, Fara, in order to illustrate a different point, quotes from the *Encyclopædia Britannica* a report of a “pure white”—that is, unstriped—tiger.

though she takes her remark to apply also to the generic quantifier²¹) that “It is unlikely that we could use such precise mathematical notions [as the set-theoretical ones used e.g. in the truth conditions of existential generalisations] to give truth conditions for sentences with adverbs of quantification... since adverbs of quantification are usually vague” (p. 32). Now, it does not seem to me apt to say that the problem with trying to account for the semantics of generics in terms of “precise mathematical notions” is that the generic quantifier is *vague*. That we cannot characterise the truth condition of a generic sentence in terms of an application of set-theoretical concepts to the set of entities satisfying its restrictor, it is quite plausible to suppose, is not because the quantity of restrictor-satisfiers said by the generic to satisfy the matrix is *vague*, but rather because a generic sentence doesn’t make a *quantificational* statement at all (though it is perhaps a little early in our enquiry to be insisting on this). Indeed Fara in effect admits as much in the next sentences, quoted already above: “Whether a sentence ‘ $[Gen\ x : \Phi x](\Psi x)$ ’ is true is not just a matter of the number or proportion of things in the extension of Φ that are in the extension of Ψ . The generic quantifier is used to make *law-like* generalizations” (p. 32, emphasis hers). That last, positive characterisation of the nature of generic generalisations—as law-like—is of course not so much an illuminating remark on their semantics as a renaming of the problem. But again, we can hardly begrudge Fara this, since she herself concedes in this paragraph that her analysis leaves the issue of the semantics of generics open. The point is simply that we, in the present enquiry, cannot rest content with Fara’s analysis.

It is also commendable that Fara, when she goes on to raise the question of the semantics of sentences such as ‘Guppies give live birth’, does not conflate this question with that of the semantics of generic sentences generally. She recognises that, although an account of quantifier

²¹ especially since, as mentioned briefly above, Fara is partial to the supposition that the generic quantifier (as well as the existential) is in fact profitably conceived of as an unpronounced adverb of quantification

domain restriction such as Stanley and Szabó's (2000) might help with this sort of sentence (since the sentence seems only to apply to guppies that parturiate, rather than to all guppies), there's no reason to suppose that it should for all generic sentences. This puts her ahead of advocates of the "relevant quantification" approach, which I shall discuss below, which offers an account of generic sentences as a whole on the basis of something close to quantifier domain restriction. However, this insight of hers is plainly a merely negative point with respect to the semantics of generics generally; and even with respect to the appeal to quantifier domain restriction, it is so far a mere promise, not a detailed account. (She has not told us how to extract the restriction from sentences such as these—how to give a systematic account of what it is about 'Guppies give live birth', for instance, that settles that the quantification is restricted to parturiating guppies—and it is worth pointing out that Fara herself says nothing about how to distinguish "sentences such as these" from other generic sentences in the first place; nothing in her account so far could serve to do so.)

Finally, I hope it is not excessively mean-spirited to wonder whether, as long as Fara has not given us a full semantic analysis of the constructions she describes, she is really entitled to her claim that they represent the structure of the sentences she's considering at the level of Logical Form. After all, the very idea of Logical Form is the idea of a level of syntactic analysis at which, to quote a standard treatment, "all grammatical structure relevant to semantic interpretation is provided" (Hornstein, 1995, p. 3); and as long as she leaves *Gen* as a black box, we seem to have no guarantee that her analyses fit this description. And in case the last few paragraphs really do seem unfair, in that I take Fara to task for not doing what *I* want her to do—*viz.*, account for the semantics of generic sentences—let the reader be reminded that the our earlier critical remarks about the odd and unexplained asymmetry her theory implies between

generics in simple definite descriptions and all others suggests that the problem is not simply that she's *missing* something, namely an account of the semantics of generics which could just be grafted on to her theory later, but rather that she has gone substantively wrong already.

§2.3. Cohen on bare plurals.

Ariel Cohen, as we have remarked above, is one of the scholars of generics who begins by distinguishing among them and treating different subclasses of them differently.²² As a matter of fact, his article “On the generic use of indefinite singulars” (2001) provides an example both of the subdivision of the class of generics on surface-structural grounds and of the treatment of syntactically parallel generics differently on semantic grounds. He argues, first, that the expression of generics using indefinite singular noun phrases (e.g., ‘A stork has a favourite nesting area’) is of much more limited applicability than the expression of generics using bare plural noun phrases (e.g., ‘Storks have a favourite nesting area’). And he further maintains that, while generic sentences expressed with bare plurals are ambiguous between what he calls (adapting terminology from Carlson (1995)) “rules-and-regulations” and “inductivist” readings, generic sentences expressed with indefinite singulars can only be given the “rules-and-regulations” reading.²³

²² Fara ends up with a similar distinction, as we just saw in our consideration of her divergent treatments of definite and indefinite singular generics. I single Cohen out as a clearer example only because he makes his distinction between indefinite singular and bare plural generics central to his account, whereas in Fara's case the distinction between definite and indefinite singular generics fell out of her account, its counterintuitiveness unremarked, rather than driving it from the beginning.

²³ Carlson's paper discussed the “inductivist” and “rules-and-regulations” approaches as two different theories of generic sentences (and advocated the second over the first). Cohen is here recommending that we recognise that generics in bare plurals (though not those in indefinite singulars) are ambiguous, in such a way that the two approaches are each applicable, but to different readings of the sentences in question.

Let me just observe here that the “inductivist”/“rules-and-regulations” dichotomy presents in interesting microcosm the problem of natural-historical judgments: for on the one hand, their factual character (as natural history) leads one to want something like an “inductivist” approach, while their admission of exceptions—which sometimes vastly outnumber the unexceptional instances—suggest some sort of “normative” or “regulative” character. It could be that the first tendency inclines us toward “distributive” approaches and the second toward “subject-predicate” approaches. However—to anticipate a little—what is missing in each type of approach is an

Let us begin by considering Cohen’s treatment of the “inductivist” reading of generics expressed with bare plural noun phrases; next we shall discuss his understanding of the “rules-and-regulations” reading of generic sentences.

Cohen’s account of “bare plural” generics might be considered to represent some advance over Fara’s initially similar treatment of definite singular generics: for while (as we saw) Fara was content to assert that ‘The tiger is striped’ should be analysed as “ $[\exists x : x \text{ is the tiger}](x \text{ is striped})$ ”, true just in case the kind *tiger* is striped, Cohen observes that a similar analysis of bare plural generics would yield semantically unacceptable interpretations. (Presumably he would say the same about the definite singular generics Fara is discussing, but they are not the topic of his work.) He points out that “[m]any of the generic sentences postulated to be cases of direct kind predication are intuitively about individuals, not kinds” (186), and that, for instance, if ‘Kings are generous’ is interpreted as a direct predication of the kind *king*, “it cannot receive a sensible interpretation” (189). He argues that, as a matter of fact, this sort of interpretation *is* what “is generated by the grammar”, for he is convinced that bare plurals do unambiguously denote kinds²⁴; but because of the semantic anomalousness, we “accommodate” (presumably in the sense of Lewis (1979)) a generic quantifier, and take the kind in question (call it *K*) as the restrictor of the quantifier, type-shifting it into the open sentence $C(x, K)$ asserting that *x* instantiates the kind (since the restrictor of a quantifier must be an open sentence, not an individual), yielding ultimately an analysis such as:

$$\text{gen}_x[C(x, \textit{tiger})][\textit{striped}(x)]$$

appreciation of the attractions of the other. Natural-historical judgments are, if you like, *at once* “factual” and “regulative” (in some sense of ‘regulative’).

²⁴ due to what he calls (crediting Erteschik-Shir) the “topic constraint”, namely that all sentences must have a topic, where a topic in turn must be something specific, such as an individual or a particular kind

This story, as we see, is rather complicated. In particular, it strikes me as odd that he insists on holding on to the claim that the bare plural “always refers to a kind” (189) even in the face of this type shifting. He presents this as a virtue of his account, since it allows him to explain sentences such as ‘Giant pandas, which are endangered, feed on bamboo shoots’, where the same noun phrase seems to be both the object of a kind predication and the restrictor of a generic quantification. But in what sense *does* a bare plural “refer to a kind” when it has been type-shifted into an open sentence acting as the restrictor of a quantifier?

Complicated or not, however, we cannot rest content with this analysis. For even if it constitutes progress *vis-à-vis* Fara’s account of generic sentences involving definite descriptions, we are still left puzzled—as Fara herself observed with respect to her own (quite similar) account of generics involving indefinite descriptions—as to the truth conditions of such constructions as **gen_x**[C(*x*, *tiger*)] [**striped**(*x*)].²⁵ Cohen tells us nothing about this quantifier. We can perhaps forgive him for this, since the article under discussion sets as its task the explanation of differences in the behaviour of bare plural and indefinite singular generics, not the articulation of a full theory of the semantics of the former. But what we are enquiring into in the present work is, precisely, the possibility of a full formal account of the semantics of sentences expressing natural-historical judgments; with this article of Cohen’s, we do not yet have an answer to our question.²⁶

As mentioned above, Cohen also discusses a treatment of generic sentences expressed with indefinite singular noun phrases, also available for one reading of those expressed with bare plurals, which he calls (adapting an idea of Carlson’s) the “rules-and-regulations” reading.

²⁵ The reader will have noticed that Fara’s account of indefinite singular generics is quite similar (apart from Cohen’s interpretation of kind predication in terms of an instantiation relation between an individual and a kind) to that which Cohen gives for (one reading of) bare plural generics but deliberately *denies* of indefinite singulars. We shall consider his analysis of the latter immediately below.

²⁶ Cohen (1999) provides a more substantive account of the semantics of such “quantificational” generics; we shall consider it below.

Generic sentences on the “rules-and-regulations” reading “do not get their truth or falsity as a consequence of properties of individual instances” (p. 194); but rather are understood as of subject-predicate form—though rather anomalously so, since the subject in question, on Cohen’s account, is not a kind but a *rule*, and the predicate in a generic of this type is in fact always the same, namely, ‘ ζ is in effect’. More specifically: first, to express that a certain rule is *followed*, Cohen writes, say, “ $\text{gentleman}(x) \Rightarrow \text{open-doors-for-ladies}(x)$ ”. (Since this conception of the internal structure of a rule involves the combination of two open sentences, which in the case of interest to us, that of natural-historical judgments, will clearly be what we have been calling the restrictor and the matrix, it makes sense to consider Cohen’s “rules-and-regulations” treatment as a kind of “distributive” approach, even though the top-level structure this proposal assigns to a generic sentence is that of singular predication.) Cohen sets aside the question how to interpret the ‘ \Rightarrow ’, leaving open the possibilities that it expresses a universally generalised conditional or “something more intensional”. Next he invites us to consider an operator which, for such a description, yields the name of a rule: $!(\text{gentleman}(x) \Rightarrow \text{open-doors-for-ladies}(x))$.²⁷ Then the form of the corresponding “rules-and-regulations” generic is: “in-effect($!(\text{gentleman}(x) \Rightarrow \text{open-doors-for-ladies}(x))$)”.

First of all, though, as we see, Cohen has no account of the ‘ \Rightarrow ’, and that’s to say that there is a serious gap in his account of what a rule is, and thus of what the truth conditions of generic sentences are. Also, he deliberately shrugs off the task of giving truth conditions for ‘in-effect(ζ)’, suggesting that they may be different for different domains (biology; etiquette; he says

²⁷ Actually, Cohen says that ‘!’ should be undefined for descriptions corresponding to which there is nothing “recognized as a rule that could conceivably hold” (p. 199)—as distinct from the question whether the rule is in effect—though this seems to me very odd. His example is $!(\text{gentleman}(x) \Rightarrow \text{has-three-fingers-on-his-left-hand}(x))$; but I don’t understand why it’s wrong to find no difference between ‘A gentleman opens doors for ladies’ and ‘A gentleman has three fingers on his left hand’ (at least from a formal-semantic point of view) except that the first is perhaps true (or anyway once was) while the second is plainly false.

they are often linguistic, so that indefinite singular generics can express definitions; etc.).

Nevertheless, since he claims to find ‘in-effect(ζ)’ as part of the logical form of any generic sentence in this class, it is not clear whether we should understand Cohen as positing a univocal, general (and *logical*?) ‘in-effect’ predicate, or a family of related ones (but *how* related?).

Finally and most broadly, absent a more positive account of the nature of rules (at the very least, some details about the truth conditional contribution of ‘ \Rightarrow ’), it is not at all obvious—not even intuitively attractive—that what underlies the truth of a natural-historical judgment is a *rule*. It does not seem like a rule, or even a law of biology, that wood ducks are colourful—for (with the exception of the occasional pigmentally-challenged male, perhaps) the ducks that aren’t colourful are breaking no rule, violating no law (for females and juveniles, after all, “are not brightly coloured”).

§2.4. Relevant quantification.

We alluded above, in our discussion of Bacon, to the idea (inspired by Frege) of understanding a generic sentence as admitting of exceptions only because it leaves implicit a restriction on the range of quantification involved, such that, were the restriction to be made explicit, the sentence could be understood as expressing a strictly universal generalisation. We saw this idea alluded to again in Fara, who suggested applying it to a subclass of generic sentences (even if she did not give a systematic characterisation of the subclass in question). Krifka *et al.* (1995) discuss several versions of this approach to the semantics of generics; they attribute the first of these, which they call “relevant quantification”, to Declerck (1991). Declerck understands the restriction on the range of quantification in generics as a *pragmatic* matter. As Krifka *et al.* put it:

[W]hen a statement is made of a “set,” the hearer will use his or her world knowledge to restrict the statement to just those members of the “set” to which it can be applied in a suitable way. For example, [the sentence ‘Whales give birth to live young’] will be a predication over female, nonsterile whales, as only they could possibly give birth to live young in the first place. (45)

More generally, if we start with our preliminary analysis of the form of generic sentences as:

GEN[*x*; *y*](**Restrictor** [*x*]; **Matrix** [{*x*}; *y*])

the current proposal is to analyse them further as:

$\forall x[(\mathbf{Restrictor} [x] \ \& \ \mathbf{R}(x)) \rightarrow \exists y(\mathbf{Matrix} [{x}; y])]$

where **R** is to be understood as a restriction variable, with the semantics of a one-place predicate (in the more general case, a predicate with places up to the number of variables bound by the generic quantifier), whose interpretation is to be supplied pragmatically.

Krifka *et al.* themselves object to this account of generics on the grounds that, unsupplemented by a “theory of suitable restrictions”, it would seem to allow that virtually any sentence of generic form could be true: for nothing would prevent us from interpreting **R**, in a given case, as coextensive with the “matrix” of the generic. (Their example is ‘Whales are sick’: if we choose to interpret **R** as “sick”, the sentence comes out true, indeed as logically true.) They express skepticism about the possibility of developing such a theory of restrictions, and hence abandon the proposal as hopeless.

I’m not as impressed with this criticism as Krifka *et al.* are, at least as it stands. Declerck’s point was precisely that the nature of **R** is a *pragmatic* matter: not one for which a theory of the sort of which Krifka *et al.* appear to despair should ever have been expected. No doubt ‘Whales are sick’ would be taken as true (indeed trivially so) by anyone who interpreted **R** as “sick”; but it is

surely not unreasonable to expect that pragmatic considerations would rule out at least this sort of interpretation, even if they cannot be fully captured by a formal theory.²⁸

Nevertheless, I believe Krifka *et al.* are on to something. They want a theory of **R**, not an account according to which “anything goes,” and they fail to register that the “relevant quantification” approach, with its appeal to pragmatics, is one according to which not quite “anything goes.” But perhaps they are right nevertheless to complain about the absence of a (fuller) theory. The question, in other words, is whether quantifier domain restriction by pragmatic supplementation really is sufficient to capture the behaviour of natural-historical judgments.

The idea behind an appeal to pragmatics, generally, is the thought that the members of a certain class of sentence, if “taken literally” (in accordance with what one’s semantic theory predicts), are false, or at any rate peculiar, and that this provokes a pragmatic accommodation on the part of the hearer. Here, presumably, the thought is that generic sentences (more specifically for us, natural-historical judgments) are to be interpreted as straightforward universal generalisations—and thus as false—but that those of us with sufficient “world knowledge” to detect this peculiarity will recognise the need for a supplementation with some restriction on the domain of quantification, and indeed (in the case of fully successful communication) will recognise just what restriction is in order.

Now, Stanley and Szabó (2000) have argued quite generally against this sort of pragmatic approach to quantifier domain restriction.²⁹ (The basic idea is that sentences with more than one

²⁸ It’s not clear just what sort of “pragmatic consideration” is being envisioned; but even Grice’s coöperative principle would seem to rule out the interpretation of an utterance of a generic sentence, in the context of an ostensibly informative description of a given form of life, as a logical truth.

quantifier expression can often only be understood on the supposition that the domain restriction of one such expression is represented by an index which can be bound by another such expression. Thus (their example) ‘In most of John’s classes, he fails exactly three Frenchmen’: here the restriction on the domain of quantification underlying ‘exactly three Frenchmen’ must refer back to ‘most of John’s classes’. And they argue that to posit indices such as these is to make quantifier domain restriction a semantic, not a pragmatic, matter.) But even if we are not fully persuaded by Stanley and Szabó’s general argument, it seems to me that the idea that expressions of natural-historical judgments in particular depend on some pragmatically supplied restriction is belied by what Thompson calls (in a different context) the “transparently ‘factual’ or ‘positive’ character” (p. 291) of these judgments. Krifka *et al.* themselves elsewhere remark on the essential place of generic judgments³⁰ in our knowledge:

Much of our knowledge of the world, and many of our beliefs about the world, are couched in terms of characterizing sentences. Such sentences, we take it, are either true or false—they are *not* “indeterminate” or “figurative” or “metaphorical” or “sloppy talk”. After all, we certainly would want to count the classic *Snow is white* as having a truth value! (3; emphasis in original)

Now to claim that generic sentences include an essentially pragmatic component is of course not immediately to contradict the view here articulated. For, after all, the thought was that expressions of natural-historical judgments “taken literally” (that is, ignoring the pragmatically provided restriction on the domain of quantification) are *false*, not that they lack truth values in some “sloppy” way. But I think I see behind Krifka *et al.*’s remarks quoted here, what is certainly in the quotation from Thompson, the idea that what a body of (correct) natural-historical judgments collectively expresses—the natural history of some life form—is (not merely either true or false but) *true* of that life form. Of course, the advocate of a pragmatic

²⁹ “On quantifier domain restriction” (2000), referred to earlier in connection with Fara’s more limited appeal to restricted quantification; Stanley provides further argument in “Context and logical form” (2000).

³⁰ In the quoted passage they speak of “characterizing sentences”, not generic sentences generally; but there is no reason to suppose they wouldn’t be willing to view “sentences with kind-referring NPs” in the same way.

approach will reply that what's important in this intuition is captured by the fact that on such an approach, what such a body of judgments *communicates*, at least to a hearer with sufficient "world knowledge," is true. But implicit in this suggestion, it seems to me, is that generic sentences, understood in this way as implicit "relevant quantifications" (where the "relevant" element, which distinguishes them from straightforward universal generalisations, is supplied pragmatically), are fundamentally *dispensable*. If 'Whales give birth to live young' is simply short for 'All female whales that reproduce do so by giving birth to live young', where the expansion is supplied pragmatically in virtue of the hearer's knowledge about whales, or indeed in any other way—and if *all* expressions of natural-historical judgments are similarly short-hand forms of straightforward universal generalisations—then generics are *not* after all essential to our knowledge of the world.

Why not simply infer in the *modus ponens* here, insisting that generic sentences are tacit universal quantifications and embracing the consequence that, *qua* generic, they are inessential to our knowledge of the world? The problem with this—indeed, the problem with the whole "relevant quantification" approach—can be brought out by considering that the sentence Krifka *et al.* chose to illustrate the approach is unduly favourable to it. Generics are, essentially, generalisations that admit of exceptions. The kind of "exception" envisaged to 'Whales give birth to live young' are *whales that don't bear young*: bulls, infertile cows, and so forth. For no whale, indeed, has laid an egg. In such a case, it will appear plausible to suppose that one can if one likes spell out the restrictions on the domain of quantification, to yield a genuinely universal truth; and to suppose that we express such thoughts generically simply to avoid the hassle of spelling out the restrictions, which will be obvious to our interlocutor in any case. But there are a great many natural-historical judgments that aren't like this. Recall Frege's example,

considered in the course of the discussion of Bacon above: ‘The horse is a four-legged animal.’ It is clear that the exceptions to this generalisation are not, as they were in the whale case, individuals to whom it is antecedently obvious (to anyone with a basic acquaintance with biology) that the predicate cannot be applied. Frege’s attempt to rephrase the sentence as an explicit universal quantification brings this out: for the exclusion of horses that are not “properly constituted” *does too much*, in the sense that a great many “improperly constituted” horses have four legs, and not coincidentally.

One might try to refine Frege’s proposal: perhaps as ‘All horses *properly constituted regarding number of legs* have four legs’. But why is this (as it surely is) a better way to rephrase the original judgment than (say) ‘All horses *properly constituted regarding the number four* have four legs’? That is to say, we now need an account (and again, for the reasons adduced above, it had better not be a pragmatic one) of how the appropriate restriction is extracted from the matrix. Another way to put the point might be: we now need a new representation of natural-historical judgments, finer-grained than the division into generic quantifier, restrictor and matrix, in order to transform them into restricted universal quantifications: we need to be able to divide what was the matrix into that part of it which contributes to determining the restriction on the universal quantification, and that part which becomes the consequent of the new universally quantified conditional. Needless to say, *how* this is to be done is something left entirely obscure by the proposal we are considering. Indeed it is not even clear that this is a coherent idea: for ‘number of legs’ is not after all in any obvious sense a *part* of, say, ‘ $\exists y_1 y_2 y_3 y_4 (y_1 \dots y_4 \text{ are distinct legs and } x \text{ has } y_1 \text{ and } \dots \text{ and } x \text{ has } y_4)$ ’. Even supposing that we could make out such a sense of “part”, it is by no means clear that in that *same* sense of “part”, something like ‘reproduction’ or ‘parturition’ (or even ‘number and colour of eggs’) will be a part of ‘ $\exists y_1 \dots y_{10} (y_1 \dots y_{10} \text{ are olive-}$

buff eggs and x lays $y_1 \dots y_{10}$ ’. But if we have no systematic means for the recovery of the restriction from (what we have until now been calling) the matrix of the natural-historical judgment, applicable both to ‘The horse is a four-legged animal’ and ‘The ring-necked pheasant lays ten olive-buff eggs’, we likewise have no systematic formal account of the semantics of natural-historical judgments.³¹

In short, although the “relevant quantification” approach appears to make progress with respect to Fara’s account in so far as it attempts to provide an analysis of the generic quantifier, whereas Fara explicitly left that as an open question, in fact the details of the analysis offered by the “relevant quantification” approach are such as to reveal that, after all, Fara’s agnosticism was well motivated: for she noticed, what the advocates of “relevant quantification” did not, that not all generic sentences, indeed not even all sentences expressing natural-historical judgments, are even plausibly thought of as universal quantifications over an antecedently specifiable subclass of the class of entities satisfying the restrictor of the original generic sentence.

³¹ Thompson, “The representation of life,” §4.3 (“Is natural-historical judgement ‘normative’ judgement?”) works through a somewhat different attempt to find a successful reductive proposal in Frege’s ‘All properly constituted horses are four-legged animals’. His main objection is that no account of the “normativity” involved can be given that doesn’t itself draw on an understanding of the life form that is the subject of the judgment. But this on its own seems unlikely to move a formal semanticist; her goal is perhaps not to give a philosophical account of the nature of the normativity in question, but to indicate what elements compose the judgment; so in the face of Thompson’s claim that the “normativity” makes ineliminable reference to the life form, she can simply represent the restriction in question as a function of the restrictor (as well as of the matrix)—as she would have done anyway.

Thompson also points out at the end of the section that on such an approach, “[t]he individual variable, and the quantifier that binds it, are... wheels turning idly” (290), in the sense that the essential elements in Frege’s formulation are simply the kind (‘horse’), the predicate (‘four-legged’) and the “normativity”. This thought, I think, is in quite the same spirit as the remarks with which I closed §2.1 on Bacon, to the effect that “distributive” approaches misguidedly locate the force of natural-historical judgments in individuals considered severally, and thereby miss the unity they express. But again, the mere claim that the quantificational apparatus is idle will not impress a formal semanticist, especially in the absence of a concrete alternative proposal; she needs to be shown that the quantificational apparatus *gets something wrong*. (Cf. footnote 49 below, in which I compare my remarks on the situation-semantic approach to natural-historical judgments to Thompson’s discussion of *ceteris paribus* clauses.)

§2.5. Prototypes.

Krifka *et al.* follow their discussion of the “relevant quantification” approach to generic sentences with a discussion of what they call the “prototype” approach. With roots in an early work of Carl Hempel and Paul Oppenheim, and taking more recent inspiration from cognitive psychology, this approach³² treats generic sentences as involving a tacit operator on predicates “which restricts the extension of a predicate to the entities that are ‘prototypical’ for that predicate” (p. 46). We can recognise this as another version of the Fregean idea (which we first encountered in our discussion of Bacon) that a generic sentence admits of exceptions only because it leaves implicit a restriction on the range of quantification involved, such that, were the restriction to be made explicit, the sentence could be understood as expressing a strictly universal generalisation. Here the restriction is interpreted as an operator on predicates, in contrast to the “relevant quantification” approach, which presented it as a further predicate conjoined to the restrictor of the quantification. This approach would render the sentence ‘A tiger is striped’ as $\forall x[(\mathbf{TYP}(\mathbf{tiger}))(x) \rightarrow x \text{ is striped}]$. More generally, the schema for generic sentences Krifka *et al.* began with,

$$\mathbf{GEN}[x; y](\mathbf{Restrictor} [x]; \mathbf{Matrix} [\{x\}; y])$$

would be defined as:

$$\forall x[(\mathbf{TYP}(\lambda x \mathbf{Restrictor}[x]))(x) \rightarrow \exists y \mathbf{Matrix}[\{x\}; y]]$$

The reader may wonder, at first, how different this proposal is from the previous one, at least at the level of formal analysis: for if (as both Heyer and Krifka *et al.* presuppose) the predicate operator in question here always yields a subset of the extension of the predicate on which it

³² Krifka *et al.* cite Platteau, Nunberg and Pan, and Heyer as advocates of this approach, but add that in their presentation, which is closest to Heyer’s, they are modifying the details in order to render it parallel to their other discussions. I shall follow them, for the same reason.

operates, the effect would seem to be the same as that achieved simply by conjoining a further condition to the predicate in question, as the “relevant quantification” approach advocated. Are prototypes not, in effect, just “relevant entities”?

But the formal difference between conjunction and predicate operation emerges when we allow the operators in question to be *intensional*—for, while the set of objects satisfying ‘ $F(x) \ \& \ R(x)$ ’ will be identical to the set of objects satisfying ‘ $G(x) \ \& \ R(x)$ ’ whenever **F** and **G** are coextensive, to say that **TYP** is intensional is just to say that **TYP(F)** need not be coextensive with **TYP(G)** even in such a case. And indeed, Krifka *et al.*, immediately after their initial presentation of the “prototype” approach, point out that the **TYP** operator will have to be intensional, simply because the envisioned generic quantifier to be defined in terms of **TYP** must itself be intensional. (Their favourite illustration is that ‘Birds fly’ had better remain true even in a case where all birds except penguins have become extinct; and they put the point here also in terms of prototypes: even in the envisioned apocalyptic case, a typical penguin would not be a typical bird.) In accommodating this sort of nonextensionality, the “prototype” approach is a bit more flexible than the “relevant quantification” approach.³³

However, this advance does not save the approach from objections. Krifka *et al.* go on to point out that a prototype operator as described so far can’t yield a satisfactory account of generic sentences, even if it’s construed intensionally. For, first, the advocates of the “prototype” approach do not tell us anything further about the semantics of **TYP** in turn: and that’s to say that all they’ve done is to convert the problem of the semantics of the alleged generic quantifier into

³³ Well, this is a little misleading, since in saying this I’m neglecting the *pragmatic* aspect of the “relevant quantification” approach. That is, since **R** itself is not constant, but is rather determined pragmatically, it could well be (indeed it is surely likely that a proponent of the approach would intend) that its interpretation vary not only with the extension of the matrix but with its intension (or its sense). Still, perhaps it can be said that treating genericity in terms of an operator on predicates seems to force us more immediately to come to terms with its intensionality, whereas the approach conjoining **R** to the explicit restrictor tends to draw our attention away from that intensionality.

the problem of the semantics of **TYP**. And then Krifka *et al.* give two examples illustrating how difficult it will be to go on to give a more substantive account of **TYP**: first, the pair of sentences ‘A wood duck has colourful feathers’ and ‘A wood duck lays whitish eggs’, which are both true, though no “prototypical” wood duck satisfies the matrices of both of these quantifications (since it is the males that have colourful feathers).³⁴ Likewise, they point out that, arguably, no human being is “typical” in every respect—but then the class of prototypical human beings is empty, making all universally quantified conditionals with ‘**TYP(Human being(x))**’ as their antecedent trivially true. More generally, we might say that no organism at all is “prototypical” in the sense of instantiating all the properties a natural history of its species would include; indeed Thompson expressed the same point more concisely: “Nobody’s perfect.”

Krifka *et al.* describe briefly three ways of modifying the “prototype” approach in the face of these objections, without critically evaluating any of them (beyond hinting that the modifications would likely detract “from the initial plausibility of prototypes” (p. 48)). The first and most telegraphic they put as follows: “Clearly, the notion of prototypicality must be relativized to the property being expressed in order to save this approach” (p. 47). The second is a suggestion that a theory of objects with conflicting properties (e.g. maleness and femaleness, in the duck example), such (they suggest) as Kit Fine’s theory of arbitrary objects, might be applied to prototypes, presumably to explain how, for instance, a prototypical duck might be both colourfully feathered and an egg layer, and hence both male and female. The third is a similar suggestion, that a theory such as Landman’s of partial or underspecified objects, by allowing that “a prototypical object can lack specific properties in one information state, but can acquire these

³⁴ I have altered slightly Krifka *et al.*’s example sentences so as to make them expressions of natural-historical judgments.

properties in more detailed information states” (p. 48), might analogously avoid the sort of contradiction they illustrated with the duck example.

I am least clear on how this third suggestion can help us, unless it is also meant to involve the second (or the first, which I will get to in a moment). The basic idea behind the “prototype” approach is to understand generic sentences as universal quantifications over prototypical exemplars of the restrictor of the generic sentence. If one of our difficulties with them (illustrated by the duck case) is that we can find sets of true generics about a given species the matrices of which do not all hold of any single member of that species, how will it help to *abstract* from some of the properties of the putative prototypes? Consider the duck example again. If ‘Wood ducks have colourful feathers’ is to come out true, it must be that all of the prototypical wood ducks have colourful feathers. The worry was that female ducks *don’t* have colourful feathers, so this sentence seemed to require that the prototypical ducks all be male. Perhaps we are to consider “underspecified” ducks, such that (in the “information state” relevant to the evaluation of this sentence) their gender is left unspecified—but they had better nonetheless all have colourful feathers. Likewise, for the sentence ‘Wood ducks lay whitish eggs’ to come out true, the prototypes, however underspecified, had better all lay whitish eggs. The suggestion was that “a prototypical object can lack specific properties in one information state, but can acquire these properties in more detailed information states” (p. 48); I suppose that the prototypical ducks, then, lack the property of egg-laying in one “information state”—where they happen to have the property of having colourful feathers—but “acquire” the property of egg-laying in another “information state” (where, presumably, they lose the property of having colourful feathers). But I cannot see how this is not simply to restate the problem of contradictory properties in picturesque (indeed utterly mysterious) terms. For, first, it seems to

me that the appeal to “information states” is idle. This can be made acute by considering the sentence (surely true if our example sentences are³⁵), ‘Wood ducks have colourful feathers and lay whitish eggs’. In what “information state” will the prototypical ducks support *this* claim? But now, once we have purged the proposal of its appeal to information states, all we have is the assertion that our prototypes both have and lack certain properties: hardly a solution to the initial problem.

The second suggestion, that the prototypes of a species might be treated as “arbitrary objects” in the sense of Fine’s *Reasoning with arbitrary objects* (1985), is an improvement over the one just considered, in that it embraces the consequence that prototypes must have conflicting properties, and recognises the need for giving an account of this consequence rather than simply renaming it. However, if I am not mistaken, it rests on a fundamental misunderstanding of Fine’s work, and is ultimately of no help in the face of the objection to prototype theory considered by Krifka *et al.*.. Fine developed his account of arbitrary objects to function as a rigorous theory limning more closely what he took to be the intuitive content of the rules of universal generalisation and existential instantiation (as they tend to be presented in natural-deductive treatments of first-order logic). On Fine’s account, the intuitive idea of “choosing an arbitrary object” is represented formally by supplementing the standard model-theoretic semantics for classical first-order logic with a set disjoint from the domain, the set of arbitrary objects, as well as a set of partial functions from the set of arbitrary objects into the domain.³⁶ The latter determines, for each arbitrary object, its range, that is, the objects in the (standard) domain (which Fine calls “individual objects”, to contrast with the arbitrary objects) for which it goes proxy; thus, an

³⁵ Thompson endorses this inference at (1995: 285), as we saw in the quotation at the opening of this chapter.

³⁶ I am abstracting from complications in the account (such as the dependency relation on arbitrary objects) which don’t concern the point about its applicability to the prototype approach to generic sentences.

“arbitrary integer” would have in its range all the individual integers, and this would be modeled by there being, in the set of partial functions, for every individual integer a function yielding that individual integer as value for that arbitrary integer as argument. As for how sentences involving arbitrary objects are to be understood, Fine explains: “An arbitrary object has those properties common to the individual objects in its range. So an arbitrary number is odd or even... since each individual number is odd or even” (1985, p. 5).

The reader will likely now be puzzled—and rightly so—about how Fine’s account yields a conception of objects with conflicting properties, as Krifka *et al.* suggested. According to their gloss of his theory, “arbitrary numbers are allowed to be even and odd at the same time” (1995, pp. 47-48); but it is clear from the sentences just quoted from Fine that this is not at all so: arbitrary integers are “even *or* odd”, since every integer is even or odd; they are not even *and* odd, since not every integer—indeed no integer—is even and odd. (The niceties of how Fine deals with the puzzle of an arbitrary integer’s being “even or odd” without either being even or being odd—since after all not every integer is even, nor is every one odd—need not delay us here, since it is beside our point.) And there is a good reason for this: Fine, again, is trying to model the reasoning involved in *universal generalisation*³⁷: if we can show that a given arbitrary object must satisfy some formula, we infer that *every* object in its range satisfies that formula. But this will be of no help for our problematic ducks. An “arbitrary duck” will be male or female, and (it may be granted) an arbitrary *prototypical* duck will in addition be colourfully-feathered or whitish-egg-laying; but it will not follow on Fine’s account of arbitrary objects that an arbitrary prototypical duck is colourfully feathered, nor that it lays whitish eggs. The latter,

³⁷ and existential instantiation (as noted above), as well as other phenomena; I single out the case of universal instantiation because it illustrates most clearly why Fine’s account is quite unsuited to play the rôle Krifka *et al.* propose for it.

for instance, would only follow, given Fine's truth conditions for predications of arbitrary objects, if *all* prototypical ducks laid whitish eggs—but the very point of the duck example was that they *mustn't* all do so, since some prototypical ducks must have colourful feathers, and hence be male, and hence not lay eggs at all. Since Fine's arbitrary objects were designed to act as bases for universal generalisation, they cannot help with the problem raised by generic sentences: namely, that two such sentences can be true of a given kind even though no one instance of the kind instantiates both of the sentences' matrices. This is, after all, a symptom of the more fundamental fact about generics that they admit of exceptions: in other words, that they aren't universal generalisations.

Was Krifka *et al.*'s reference to Fine simply a gaffe to be eliminated and replaced with a more suitable theory—that is, with something that genuinely yields an account of conflicting properties? (Indeed, they introduce Fine's work merely as an example of such a theory, suggesting that they would be as happy with any other.) First of all, if so, it's clear that this suggestion of how to mend the prototype approach to generic sentences amounts, at best, to a promissory note: not to a positive account of the semantics of generics. But further, and more gravely: can there *be* a suitable theory of conflicting properties? What this type of response to the duck example requires is that the prototypical ducks—or at any rate the arbitrary prototypical ducks, or (since that word is already taken) perhaps we should call them the conflict-ridden prototypical ducks—be *at once* male and female, colourfully feathered and whitish-egg-laying. But how will a theory countenancing such entities then prevent us from inferring that a typical duck is one which both is colourfully feathered and lays whitish eggs? It seems that the theory, to meet the objection, would have somehow to partition the properties exhibited by the conflict-ridden prototypical ducks into groups that go together. But then how would this be significantly

different from simply recognising the necessity for at least two distinct prototypes? In neither case does it obtain, as the essence of the prototype approach to generics requires, that a generic sentence is true just in case its matrix holds of *all* of the prototypical instances of its restrictor.

In the light of this last objection, perhaps (and here I go beyond anything Krifka *et al.* even hint at) we could imagine reconceiving **TYP** so as to yield, when fed a kind as argument, a *set* of (*distinct*) prototypes of that kind: for instance, a female duck that lays whitish eggs, a male duck with colourful feathers, and so on. (We might imagine that it is the task of the natural historian to determine just how many such prototypes suffice to encapsulate a given species's natural history: a species that goes through many metamorphoses might require a great many; some simple asexual life form might, we could perhaps imagine, require only one.) We could then revise the truth definition for **GEN** by replacing Krifka *et al.*'s universal quantifier with an existential: such that a generic sentence is true if and only if *some* prototype of its restrictor satisfies its matrix. Unfortunately, however, this too will not be true to the semantics of generic sentences. Returning to our ducks, the prototype laying whitish eggs will after all be female, and our truth definition will therefore validate 'A wood duck is female', a manifest falsity.

(Similarly with life forms exhibiting metamorphosis: this is usually tied to the age of such creatures, but for instance, even if the tadpoles of a certain species of frog metamorphose into adults after fewer than three months, so that the prototype(s) exhibiting the tadpole stage of that frog species will be less than three months old, 'A frog is less than three months old' is clearly false, again in violation of the semantics we are considering.)

Now, though, it may seem to help to revive the idea of deploying a theory of "partial objects", such as Landman's, against whose usefulness for our purposes I argued above. It was the combination of partiality and universality (to put it telegraphically) that led to problems there;

now that we have considered revising the prototype approach so as to understand generic sentences as existential rather than universal quantifications over prototypes, the account may seem more promising. Thus we can again imagine the natural scientist providing us with our several prototypes for a given species, but omitting such predicates from the description of each prototype as would yield false generic sentences such as ‘A wood duck is female’ or ‘A frog is less than three months old’. —But this procedure, in turn, is awfully hard to envision in any more detail than I have just sketched. For after all, the ducks that lay whitish eggs *are* female: this is every bit as much a truth about the natural history of ducks as the fact that they lay whitish eggs itself. Landman’s account of partiality appealed to information states—but in what “information state” do we know that a certain duck lays eggs but not that it’s female? How plausible can it be that a grasp of the truth conditions of ‘A wood duck lays whitish eggs’ depends on feigning ignorance of the fact that only female ducks lay eggs (since from the generic together with that knowledge we could, on this proposal, infer ‘A wood duck is female’)?

But I have so far neglected Krifka *et al.*’s first suggestion for dealing with the wood ducks and the typical human beings: to quote it again, “Clearly, the notion of prototypicality must be relativized to the property being expressed in order to save this approach” (1995, p. 47). I take this rather cryptic formulation to mean that we are to conceive of **TYP** as a function of two arguments—the restrictor and the matrix—rather than just of the restrictor. Thus ‘A wood duck has colourful feathers’ would be interpreted as something like “Every wood duck typical *with respect to having colourful feathers* has colourful feathers.” I have neglected this suggestion in my discussion of the proposals to employ theories of partial objects, or objects with conflicting properties, mainly because I cannot see, actually, why the latter theories would still be necessary if this suggestion were successfully implemented: for there is no contradiction between “Every

wood duck typical with respect to having colourful feathers has colourful feathers” and “Every wood duck typical with respect to laying whitish eggs lays whitish eggs” (no reason that the ducks quantified over by the former shouldn’t be male and those by the latter female); and there is not such a pressing worry that the set of, say, human beings typical in some one respect is empty as there was that the set of human beings typical in *every* respect might be empty. The question here is simply how the suggestion can possibly be implemented. The interpretations I have provided of the sentences on this new conception of **TYP** as a two-place function sound rather close to trivial, and it might be thought that I should have said, for example, “Every wood duck typical with respect to laying eggs lays whitish eggs.” But it is not at all clear what systematic account of the behaviour of **TYP** would explain how that is to be derived from ‘A wood duck lays whitish eggs’. In other words, the last problem we discussed for the “relevant quantification” approach, §2.4—the problem of extracting from the matrix just the information needed for the restriction on the quantification—applies here as well. On the other hand, we may decide (perhaps in the face of this consideration) that my original interpretations, albeit peculiar-sounding, were nevertheless correct: we may hold, for instance, that the true ‘A wood duck lays whitish eggs’ means “Every wood duck typical with respect to laying whitish eggs lays whitish eggs” while the false ‘A wood duck lays brown eggs’ means “Every wood duck typical with respect to laying brown eggs lays brown eggs,” the latter being false just because (even if there are occasional mutant wood ducks laying brown eggs) what it is for a wood duck to be typical with respect to laying brown eggs is for it to lay whitish eggs. But surely it is clear that this is simply burying the problem just discussed, of how to extract something like “laying eggs” (*tout court*) from ‘lays whitish [or brownish] eggs’, rather than actually solving it, since the reason *why* “what it is for a wood duck to be typical with respect to laying brown eggs is for it to lay

whitish eggs” is plainly that laying brownish eggs is a way of *laying eggs*, and to be typical with respect to that, for a wood duck, is to lay whitish eggs.

(A related problem arises when we consider simple matrices, such as that in ‘A wood duck is female’. On the present proposal, we are to understand this as “Every wood duck typical with respect to being female is female.” It is rather hard to imagine what account of **TYP** would fail to make the latter come out true—though the former is clearly false.)

So much for the prototype approach.

§2.6. Stereotypes.

The idea behind the stereotype approach to generic sentences is that, for instance, the reason for which ‘A lion has a mane’ is a true generic sentence but ‘A lion is male’ is not, though all lions with manes are male, is that having a mane is part of our (linguistic, cultural) *stereotype* of a lion (in a way in which maleness of course isn’t). Krifka *et al.*, in objection to this proposal, emphasise what Thompson calls the “transparently ‘factual’ or ‘positive’ character” (1995: 291) of natural-historical judgments, giving as an example ‘Snakes are slimy’, which is false regardless how widespread any stereotype to the contrary might be.³⁸ Krifka *et al.* also point out that, since “there is little hope that we will find principles of general logical interest” underlying the formation of stereotypes, the interpretation of the generic quantifier in terms of stereotypes will yield “no general theory of the semantics of **GEN**” (1995: 48, 49). Finally, they argue that the idea of stereotypes cannot provide a general account of **GEN**, since there are true generics whose restrictors involve complex subject classes too specific for there to be cultural or linguistic stereotypes associated with them.

³⁸ We noted Krifka *et al.*’s commitment to this “realism” (as we might call it) about generics above, in our discussion of the “relevant quantification” approach, although they didn’t deploy it as an objection there (as we did).

I believe that Krifka *et al.*'s first objection is the deal-killer: even if some classes of generic sentence are tied to stereotypes in some way, the generic sentences in which we are interested in this enquiry—those expressing the natural history of a species—need have no connection with such sociological phenomena. The observation that a stereotype approach to generics is unlikely to yield a formal-semantic theory is perhaps a less compelling objection, since an advocate of the approach might simply shrug and conclude that what's special about generics is precisely *not* “of general logical interest.” (And we in this enquiry may feel more of an obligation to take this possibility seriously than the average linguist or formal semanticist.) The point about complex subject classes may also be of less relevance to us than to a scholar of generics in general: for Thompson's conception of the expression of a natural-historical judgment is, paradigmatically, as the combination of a *life form word* with a predicate, and a life form word is presumably semantically simple. This is not to say that natural-historical judgments with complex subject terms are unthinkable—it is surely also true that the male lion has a mane—but one might find implicit in Thompson's remarks a suggestion that such judgments are in some sense posterior to ones made about a given life form *tout court*.³⁹ If we are to take seriously Thompson's claim that natural-historical judgments are unique, then we must also take seriously theories of generics which, even though they clearly don't apply to generics across the board, seem to hold some promise for expressions of natural-historical judgments in particular. However, again, of course, not all natural-historical judgments, indeed not even all of them with simple subject terms, can be understood as resting for their truth on stereotypes, for the reason already mentioned. In short, it's Krifka *et al.*'s first objection that warrants the rejection of the “stereotype” approach to natural-historical judgments.

³⁹ E.J. Lowe (1989) gives a general theory of sortal predication in which reference to complex sortals is in all contexts eliminable in favour of reference to simple sortals.

§2.7. Koslicki's "two-tiered" approach.

Koslicki (1999) considers and rejects both subject-predicate and relational (that is, in our terms, "distributive") analyses of generic sentences. She rejects the former on the grounds that some generics—generics whose predicates appear to attach more naturally to individuals, or "I-generics" in Krifka's language (mentioned at note 2 above)—have scopal properties that seem to be excluded by such analyses. And she rejects the latter since *other* generics—"D-generics", as Krifka calls them, such as predications of extinction, rarity and so on—seem unamenable to them. She also rejects "non-uniform" analyses which take the former of these two types of approach for the sentences for which it appears to work (D-generics) and the latter for the sentences for which *it* appears to work (I-generics), because this requires positing a systematic ambiguity in the noun phrases appearing in such sentences. (On such an approach, 'The tyrannosaur' in 'The tyrannosaur is extinct' denotes a kind, but in 'The tyrannosaur was warm-blooded' it serves to express quantification over individuals of that kind.) She recommends, instead, a "two-tiered" approach, involving a first level where all generics receive a parallel analysis in terms of plurals (understood, drawing on Boolos's work and also on Higginbotham and Schein, as referring not to plural objects but to "Fregean concepts"), and a second, lexical level where plural predication itself is analysed, differently for different lexical contexts. (Thus what it is to predicate plurally is different when the predicandum is extinction from what it is when the predicandum is warm-bloodedness.) In particular, the plural predication appearing in first-level analyses of the sorts of generic for which a "relational" analysis initially seemed plausible will indeed, Koslicki suggests, be unpacked quite uniformly in terms of a generic quantifier, while the plural predication involved in what look like kind predications will get quite heterogeneous treatments depending on just what is being predicated. She gives examples: 1. 'A

dog is a faithful pet’ and 2. ‘The dinosaur is extinct’. The first tier of analysis yields, for 1., Are-faithful-pets($(\iota X)(\forall y)[X(y) \leftrightarrow \text{Is-a-dog}(y)]$), and for 2., Are-extinct($(\iota X)(\forall y)[X(y) \leftrightarrow \text{Is-a-dinosaur}(y)]$).⁴⁰ At the second tier, since 1. can be understood as distributing the property of being a faithful pet to individual dogs, we interpret Are-faithful-pets(X) as $\text{GEN}(x)[X(x), \text{Is-a-faithful-pet}(x)]$; and for 2., we understand the plural predication quite differently, and for Are-extinct(X) we get, perhaps, $\forall x[X(x) \rightarrow \text{Is-dead}(x)]$ ⁴¹. We are to take our cue from the particular predicate (here, ‘Are-faithful-pets’ and ‘Are-extinct’ respectively) in determining how the second tier of analysis should go in a particular case: as Koslicki says, it’s a *lexical* matter.

Now, it may be that natural history has no essential need for D-generics (since facts such as that a species is extinct, or rare, or was introduced to Ireland in the 1700s or what have you, don’t seem to be part of the natural history of life forms, in the sense of ‘natural history’ at issue in natural-historical judgments). If so, the motivation to move toward non-uniform approaches, and then to Koslicki’s two-tiered approach, may to some extent be undercut, from our point of view in this enquiry as interested in natural-historical judgments.

However, let us set this aside and suppose that the motivation for developing something like Koslicki’s two-tiered analysis is compelling. (After all, even if it is true that natural-historical judgments themselves are always I-generic, there is still something to be said for the thought that the noun phrases appearing in them should be interpreted synonymously with the orthographically identical phrases appearing in (*ex hypothesi* non-natural-historical) judgments of extinction, rarity and so on.) We may nevertheless object that, though Koslicki eliminates the

⁴⁰ The ι notation here works something like lambda abstraction, so that $(\iota X)(\forall y)[X(y) \leftrightarrow \text{Is-a-dinosaur}(y)]$ is to be read as “the concept X such that something is in its extension just in case it’s a dinosaur”.

⁴¹ Koslicki gives two possible unpackings of Are-extinct(X), both different from what I give in the text; the complication arises from the temporality of extinction, and the general question how to deal logically with this temporality. However, since this is irrelevant to our present concerns, I have ignored the niceties.

ambiguity in noun phrases she worried about in association with the nonuniform approaches, she simply replaces it with an ambiguity in the very *form* of plural predication—which seems, if anything, even more disturbing. Referring to work by Higginbotham, Koslicki explains the two-tiered structure of her account in terms of the difference between the “combinatorial/structural” aspect and the lexical aspect of the theory of meaning. That generics are plural predications, then, is a matter of their logical form, she says, while how plural predication itself is to be unpacked is a lexical matter. But it strikes me as absurd to think that the transition from *Are-faithful-pets*((ιX)($\forall y$)[$X(y) \leftrightarrow \text{Is-a-dog}(y)$]) to $\text{GEN}(x)[(\iota X)(\forall y)[X(y) \leftrightarrow \text{Is-a-dog}(y)](x)$, *Is-a-faithful-pet*(x)] is a lexical matter requiring attention to the particular predicate ‘Are-faithful-pets’ rather than a matter of the form of predication involved.

Now, “it strikes me as absurd” is so far not much of an argument; and perhaps Koslicki will respond in her defense: If I’m told, ‘The Bandersnatch is frumious’, surely it’s true that I won’t know whether this is “distributive” or “kind-predicative” unless I know what ‘frumious’ means: whether, that is, it means something like ‘furry’ or something like ‘extinct’. But likewise, I don’t know whether ‘vorpall’ is an attributive adjective⁴² or not in ‘He took his vorpall sword in hand’ unless I know what ‘vorpall’ means; but that (at least arguably) doesn’t show that the attributive/nonattributive distinction is merely lexical either: contrast Evans (1976), who draws heavily on the thought, precisely, that this distinction is structural. It therefore seems possible to resist the implication of Koslicki’s analysis that the distinction between “distributive” and “kind-predicative” (I- and D-) genericity is “lexical”, even while granting the point about ‘frumious’,

⁴² “attributive” not in the sheerly grammatical sense, but in the “logical” sense of an operator on predicates rather than a predicate (the sense at issue *e.g.* in Evans (1976)).

simply by pointing out that structural features aren't necessarily visible at the surface level.⁴³ At a minimum, we might say, whether a generic is "D" or "I" can be a structural rather than a lexical feature, even if we agree with Koslicki that, if it's "D", then *how* it's "D" (so to speak) is a lexical matter.

Indeed, even before the ambiguity in the form of plural predication, notice (what Koslicki doesn't seem to notice) that her account actually posits an ambiguity in the *predicates* appearing in I- and D-generic sentences as well. Consider: her analysis of 'The tiger is striped' would yield, at the first tier (that is, at the level of logical form), 'Are-striped($((\iota X)(\forall y)[X(y) \leftrightarrow \text{Is-a-tiger}(y)])$)'. But then 'ζ is striped' as it appears in the example sentence is interpreted as 'Are-striped', which takes as its argument a *concept*—whereas in 'Tigger is striped', the argument of the predicate is an individual. Now, the second tier of Koslicki's analysis removes this formal ambiguity (since, this being an I-generic, the plural predication is reinterpreted as a predication distributed generically over individuals); but what are we to make of the fact that, again, this second tier is said to be *lexical*? At the level of logical form, it seems, a wide range of predicates are to be treated as admitting both concepts and individuals as arguments.

Indeed, it begins to seem reasonable to ask what the ground is for taking the detour through the second-order tier. Koslicki argues that, if one cuts straight to the elucidatory analyses of I-sentences—which tend to wind up as mere quantifications—one obscures what was *generic* about them. It is the first-stage, second-order analysis that brings out what they have in common with D-sentences (and so, one supposes, *vice versa*: the analysis of D-sentences as plural predications, before cashing this out in accordance with the separate demands of the various

⁴³ Compare also 'Scott is the author of *Waverley*' to 'Scott is the picture of patriotism': (though Fara might disagree,) here too we seem to have a structural difference underlying surface-syntactically parallel constructions.

predicates appearing in them, shows what they have in common with I-sentences: namely, genericity.) –But the disjunctive structure of Koslicki’s account of plural predication suggests, rather, that, contrary to appearances, I-sentences *don’t* have anything substantive in common with D-sentences. Another way of bringing this out is to consider that, if the first tier of analysis was intended to show what makes the two classes of sentence both *generic*, then it is rather too broad: for there are after all a great deal of obviously *nongeneric* sentences that share the form given at this first tier! In fact, we can illustrate this point with an example sentence that Koslicki herself adduces in the course of her account of plural predication: for ‘The rocks rained down’, she gives the analysis $\text{Rained-down}((\iota X)(\forall y)[X(y) \leftrightarrow \text{Is-a-rock}(y)])$. And there is not a trace of genericity in this sentence: this is simply a report of a one-time occurrence involving a (mere) plurality of objects. But this shows that the form of plural predication *isn’t* special to genericity. Presumably here, as in the other cases, we are to figure out how to understand this plural predication by looking at the predicate ‘Rained-down’—but then, since it’s at *this* stage where we determine whether we have i) an I-generic, ii) a D-generic, or iii) some nongeneric sentence like the one about the rocks, that is surely just to say that, on Koslicki’s account, *genericity itself* becomes a lexical matter. There really is no unifying accomplished by this first tier of analysis. Furthermore, whatever we make of the two tiers of her analysis, the fact that her account (like those of Fara and Cohen above) leaves GEN, as it occurs in the interpretation of I-generics at the second tier, unexplained means that it cannot satisfy us in our present enquiry.

Finally and most generally, the very idea (which Koslicki just tosses out casually) that “kinds, after all, are just another variety of plural objects” (1999: 453) seems fundamentally misguided. ‘Tom, Dick and Harry’, and indeed ‘this dissertation, the sun and the number 3’ perhaps denote plural objects: but it seems plain that a kind is not “just another plural object” in this sense.

What perhaps tempts people into subject-predicate types of analysis (besides surface grammar) is that (natural) kinds have a kind of *unity* that goes beyond anything countenanced by the formal treatment of plural objects (not to mention by quantification employing singular predication). Boolos himself observes about his account of plural predication, intending this as a selling point, that it brings no further ontological commitment besides that already involved in classical first-order logic.⁴⁴ So we might express the present objection, somewhat sloganistically, by saying that it is precisely this absence of further “ontological commitment” that makes Koslicki’s approach “distributive” in our sense, and also that renders it oblivious to the “unity” belonging to natural (life form) kinds at which we just gestured. (But perhaps this would be a misleading slogan, since non-nominalistic treatments of plurality, which interpret “plural objects” as sets or mereological sums or some such, really do no better at capturing the “unity” I have in mind.)

§2.8. Cohen’s “probabilistic” theory.

With this article of Cohen’s, unlike the previous one of his we considered (and unlike the accounts of Bacon, Fara and Koslicki), we have a substantive attempt at a semantics for a generic quantifier. Unfortunately, it’s flawed.

Cohen begins by proposing to treat his “generic operator” (which he writes as ‘Gn’) as operating simply on pairs of predicates, not on open sentences; there are thus no variables ranging over individuals in his representations of generic sentences, and this may make his proposal seem not quite “distributive” in our sense. However, he suggests in a footnote that his treatment could be broadened to allow the operator to take open sentences as arguments, and in any case the truth conditions he settles on for generics involve probability understood in terms of relative

⁴⁴ Because of this, it is in fact a little puzzling that Koslicki persists in talking of “plural objects”: on the treatment of plural predication which she endorses, it involves no new *objects*.

frequency—that is, of course, relative frequency of *instances*, which brings in explicit reference to the satisfaction of the predicates by individuals—so it seems reasonable to treat his proposal as a version of the “distributive” approach after all.

Cohen compares generic sentences with sentences involving frequency adverbs, and he proposes a unified treatment of both classes of sentence as probability statements. In essence, the idea is to treat a generic as true just in case the probability of an entity’s satisfying the matrix given that it satisfies the restrictor is greater than one half. He recognises that such an account will not do without further refinements; as he puts it, in the case of generics, “majority is not enough” (1999: 228). So he introduces an account of probability as hypothetical relative frequency, based on von Mises’s treatment, but with some refinements. (We are to consider relative frequency in arbitrary “histories” which are extensions of the actual one and for which no temporal partitioning of the domain yields differing relative frequencies in different partitions.) Cohen declares, first, that sentences in ‘most’ and ‘usually’ have as truth conditions that the probability, in the sense just sketched, of a restrictor-satisfier’s satisfying the matrix of the sentence is greater than one half.⁴⁵ To distinguish generic sentences from such ‘most’ sentences, Cohen simply imposes some further conditions on the partitions deemed relevant to the probability calculation: a generic sentence is true only if the frequency in such histories of matrix-satisfiers among restrictor-satisfiers is homogeneous not only with respect to time but also with respect to any other partitions of the domain pragmatically deemed “*salient*, given the context and the language user’s model of the world” (242; Cohen’s emphasis).

⁴⁵ He suggests that such sentences carry an implicature that the probability is *substantially* greater than one half, but insists that such sentences are literally true even when the probability is only minimally greater.

Now, Cohen starts out on the wrong foot when he says that “we are only concerned here with the *descriptive* readings of generics, and not with the *prescriptive* readings” (222; his emphasis). He says this in the context of considering that generics can be true even when they have no supporting instances: one of his examples is ‘Mary handles the mail from Antarctica’, when no such mail has ever come. The sentence proves false on the descriptive reading, but remains true on the prescriptive reading, if some mail arrives from Antarctica and Mary neglects it, though it is her job to handle it. But it is hard to see how to hold on to the idea that the reading at issue is the descriptive one at the same time as imagining that the sentence is true but as yet uninstantiated. I think this is a symptom of Cohen’s predisposition toward a probabilistic conception of generics.

The elaborate story Cohen tells about the sense of probability at issue, involving homogeneity and “salient partitions” imposed pragmatically, seems to involve telling “just so stories” in order to make the probabilities line up with what we already know about the truth conditions of generic sentences. But one is left with the sense that the true account of the semantics of generics ought to get directly at whatever it is that grounds this understanding of ours of the truth conditions, rather than taking what feels like a detour through the story about probabilities.

In addition, when we consider the aptitude of Cohen’s account for natural-historical judgments in particular, it is clear that the conception of “salient partitions” as a pragmatic matter dooms the account to an inability to register what (as we’ve seen) Thompson calls the “transparently ‘factual’ or ‘positive’ character” (1995: 291) of natural-historical judgments. Cohen emphasises the objectivity of one aspect of this notion of partitions: he writes, “While the saliency of partitions may vary across individuals and contexts, I take it to be an objective fact whether or not a given domain is homogeneous with respect to a particular set of salient partitions and a

property” (1999: 242n11). But this is not enough to save the account from the objection I am raising: for it is precisely the varying “across individuals and contexts” that causes the problem. For instance, Cohen suggests that the falsity of ‘People are over three years old’ in the face of the fact that, statistically, most people *are* over three years old, is to be explained by the fact that hearers of this sentence will evaluate it against the background of a partition according to age. However, if there were some context in which, or some speaker for whom, the partition according to age were not salient, this sentence would be true in that context or for that speaker, according to Cohen’s account of its truth conditions. But as a natural-historical judgment, this sentence is just false: it is simply not part of the natural history of human beings that we are over three years old.

In short, Cohen’s account is not of the truth of generic sentences *tout court*, but of their truth-relative-to-speaker-and-context (as he makes explicit e.g. at p. 242)—but speaker and context are simply not parameters with respect to which the truth of natural-historical judgments varies. Even if his account were satisfactory for other classes of generic sentence, in order to accommodate the phenomenon of natural-historical judgments, Cohen would need to supplement his account with, at a minimum, a theory of “salient partitions” (if “salient” would still be an appropriate label) for natural-historical judgments that would make the partitions as objective a matter as the homogeneity of a domain with respect to the partitions once given. But not only does Cohen not give us such a theory (a fact for which one might forgive him, since his target is the semantics of generic sentences as a whole, not expressions of natural-historical judgments in particular—though it is still true that he takes his account to apply *also* to the latter, and our argument shows that, as long as the question of relevant partitions is left as a “pragmatic” matter, this is false), there is good reason for despairing of the availability of such a theory. When, once

he has introduced the notion of salient partitions, Cohen talks us through several examples showing how he can squeeze the correct truth conditions for the example generic sentences from his account, it is hard to avoid the impression, again, that he is telling “just so” stories about how to partition the domain so as to yield homogeneity for the true generics and heterogeneity for the false. It is harder still to envision a *theory*—even one available in principle—which could yield the correct partitions in advance, without looking ahead to an independent grasp of the truth values of the sentences it provides partitions for.

And in any case, the account in terms of probability doesn’t get things right.⁴⁶ Cohen recognises, as we saw, that “majority is not enough” for a generic sentence to be true; thus, while frequency statements, for their truth, only require homogeneity across temporal partitions of sequences, generic statements require homogeneity across many more partitions—where “the suitability of partitions is a pragmatic matter. A partition is suitable to the extent that it is considered salient, given the context and the way we view the world” (1999: 242; emphasis removed). Cohen invokes this mechanism to rule out a range of putative counterexamples to his view: sentences for which the hypothetical relative frequency in question is greater than one half, but which plainly do not express true generics. Thus he gives the example ‘Bees are sexually sterile’: this appears to be a false generic sentence, despite the fact that, statistically speaking, most bees *are* sterile. He suggests that hearers of this sentence will evaluate it against the background of a partition of the bees by gender—or, alternatively, by reproductive ability—which will yield a heterogeneous reference class (since female bees, though less common, are

⁴⁶ This point is not really separate from that of the previous paragraph: Cohen’s discussion of “salient partitions” smacks of ad hocery, I think, precisely because what he is doing is looking for a partition on the domain yielding homogeneity with respect to the matrix of a given generic, so as to make the probabilities turn out right—where that with respect to which they are to “turn out right” is a grasp of their truth conditions which (I claim) is not really *probabilistically* grounded at all.

generally not sterile), explaining our taking the sentence to be false. He walks through several other examples, showing how an invocation of “salient partitions” can explain why generic sentences can be untrue when their corresponding frequency statements are true.

Though this mechanism is ingenious, I don’t believe that it yields a true account of generic sentences. We can begin by observing that Cohen’s slogan, that “majority is not enough”, sits uneasily with an essential fact about generics that he appears to miss: namely, that on the other hand, majority is sometimes not even a *necessary* condition. Most scholars of generics advert to this feature of them as one of their more puzzling properties near the beginning of their analyses: see Krifka *et al.* (1995) for one example, and consider (for a pithy if somewhat paradoxical expression of the same thought), Thompson’s observation that “although ‘the mayfly’ breeds shortly before dying, *most* mayflies die long before breeding” (1995: 284). Cohen’s partitions will not save his account from this flaw: for when $P(\Phi \mid \Psi)$ is less than one half, no amount of clever homogeneity-preserving partitioning of the domain will turn $P(\Phi \mid \Psi)$ into something greater than one half—for the partitions, again, must be *homogeneous*! And yet, as the case of the mayflies illustrates, the phenomenon of true generics of the form $G_n(\Psi, \Phi)$ where $P(\Phi \mid \Psi) < 0.5$ is widespread.

Cohen, perhaps inadvertently, discusses a case of this: he asks why ‘Mammals give birth to live young’ is generally taken to be true, while ‘Mammals are female’ is not, even though the probability of a mammal’s being female is actually higher than that of its giving birth to live young. (He does not explicitly note that $P(\text{Gives birth to live young} \mid \text{Is a mammal})$ is *less than one half*; he simply notes that it is less than $P(\text{Is female} \mid \text{Is a mammal})$.) He claims that “[t]he requirement of homogeneity explains [this] phenomenon” (1999: 247); but in fact his

explanation of how this is supposed to go adverts to *domain restriction*, which has nothing to do with the requirement of homogeneity. He writes:

... the property *give birth to live young* induces a set of alternative forms of procreation, e.g. {*give birth to live young*, *lay eggs*, *undergo mitosis*}. The domain of the generic is restricted to only those mammals which satisfy one of those alternatives, i.e., procreate in some fashion; this constitutes a subset of female mammals. Since a procreating mammal is highly likely to give birth to live young, [*'Mammals give birth to live young'*] is true. (247)

But this, in effect, is to confess that, contrary to his official doctrine, $G_n(\text{Is a mammal}, \text{Gives birth to live young})$ is *not* true just in case $P(\text{Gives birth to live young} \mid \text{Is a mammal})$ is greater than one half—even when the explanation of $P(\Phi \mid \Psi)$ in terms of hypothetical relative frequency is supplemented with his story about salient partitions. What his account has to be, even though he doesn't explicitly recognise this, is an account in terms of probability *nested inside an account of domain restriction*. He doesn't give us any details about this latter, equally necessary facet of his full account (though he cites previous work of his); but in any case, we have already discussed the problems associated with the attempt to explicate generic sentences in terms of quantifier domain restriction. (Note that, just as in our discussion of domain restriction in §2.4, “Relevant quantification,” Cohen's example here involves giving birth, so that his story about domain restriction is at least plausible-sounding. But (as we observed in §2.4), many of the true generics of the form $G_n(\Psi, \Phi)$ where $P(\Phi \mid \Psi) < 0.5$ are *not* such as to invite this kind of obvious domain restriction.)

§2.9. Modal approaches.

Krifka *et al.* suggest that a promising strategy for treating the semantics of generic sentences, adopted for instance by Heim, is by adapting and applying to them the modal approach to the semantics of conditional sentences (as found e.g. in Lewis, *Counterfactuals* (1973)). They make this suggestion on the grounds that generic sentences, at least of the sort that seem disposed to be

analysed distributively, “resemble conditional sentences” (1995: 49)—indeed, as they put it later, “A [generic] sentence such as ‘A lion has a bushy tail’ *expresses the same concept* as the conditional sentence ‘If something is a lion, it has a bushy tail’” (1995: 52, emphasis added).

They therefore begin by describing a standard treatment of possible-worlds semantics for modality (whose presentation is due to Kratzer), involving the \Box/\Diamond distinction, the accessibility relation among possible worlds, and an “ordering source” imposing a similarity ordering on worlds. After reinterpreting ‘A lion has a bushy tail’ as ‘If something is a lion, it has a bushy tail’, they propose to treat it as implicitly *must*-like, “in order to capture the quasi-universal force of [generic] sentences” (1995: 52). Roughly, their truth definition yields that ‘A lion has a bushy tail’ will be true in a world w with respect to a given accessibility relation and ordering source just in case “everything which is a lion in the worlds [accessible to w] is such that, in every world which is most normal according to the ordering source, it will have a bushy tail” (p. 52). The point is not that all lions in worlds accessible to w have bushy tails, but that any non-bushy-tailed lion in such a world must be in a world *less normal* than one in which it has a bushy tail.

What conception of normality (and for that matter accessibility) underlies this structure? Krifka *et al.* tell us that, since the accessibility relation and ordering source are often left implicit, “hearers construct a modal base and ordering source for the interpretation of a sentence, in order to *accommodate* it” (p. 56; italics in original, with credit to Lewis (1979) for the concept of accommodation). Their remarks on the accessibility and ordering relations appropriate for natural-historical judgments are few, and raise problems, as they themselves note: for, by their definition, the truth of ‘A turtle is long-lived’ in w seems to require that the “most normal worlds” accessible to w be ones in which all turtles survive to an old age—though this would be, in fact, a quite abnormal scenario for turtles. Likewise, ‘A pheasant lays speckled eggs’ seems to

require that all pheasants in the most normal worlds lay eggs—but then they must all be female, and (as Krifka *et al.* point out) it is puzzling how, in those worlds, they could have been fertilised.

Now, the initial assimilation of generic sentences to conditionals, meant to motivate the proposal, is controversial. Even for genuinely universal generalisations, the equivalence between the natural-language ‘All Ss are Ps’ and the (stilted) ‘Anything is such that if it’s an S, it’s a P’ is contentious. (Thus in a sortal logic such as Gupta’s *The logic of common nouns* (1980), which interprets all quantification as restricted quantification, such that we always quantify over things of a certain kind—and which eschews a universal sortal ‘thing’—this “Fregean equivalence” does not hold fully generally.) But more gravely, to the extent that the “Fregean equivalence” itself is intuitive, the assimilation of *generics* to conditionals may *not* be. For after all, the (false) universal ‘All lions have bushy tails’ is, according to the Fregean equivalence, also equivalent to ‘If something is a lion, it has a bushy tail’. Since the generic is certainly not equivalent to the universal, it doesn’t seem as though it can be equivalent to ‘If something is a lion, it has a bushy tail’ either. —Now, of course, in response to this, we’ll be urged to “hear the conditional generically”, meaning (on this approach) to hear it as a conditional understood *modally*, and in particular against the background of the appropriate modal apparatus. (Indeed advocates of modal approaches to conditionals will surely insist that this is appropriate quite generally, and that if conditionals are ever equivalent to straightforward universal generalisations as Frege held, this is to be modeled by suitably tweaking the modal apparatus.) But arguably, the assimilation to conditionals is nevertheless more contentious in the case of generics than in that of universal generalisations, for the sort of reason discussed at the end of §2.1 on Bacon: a (however modally) quantified conditional quantifies over *individuals*, and I suggested there that any

attempt to understand natural-historical judgments in terms of the properties of a selection of (now perhaps merely possible) individuals will do injustice to them. But let us turn to the details of the proposal.

As for what the appropriate modal apparatus is: Krifka *et al.*'s remarks on the pragmatic accommodation of generic sentences may make us nervous, in the light of the “transparently ‘factual’ or ‘positive’ character” Thompson attributes to natural-historical judgments (1995: 291) that we have now seen several times. On the other hand, the very exercise Krifka *et al.* go through, of working through various examples of generic sentences and cataloguing the different accessibility and ordering relations that would explain their truth conditions, suggests an interesting response to Thompson's complaint, with which we began this chapter, according to which, since generics constitute a “rag-bag” of logically distinct types of sentence, no formal treatment of their semantics can shed light on the logical form of the natural-historical judgment. In addition to the response we already gave to this objection at the beginning of this chapter—*viz.*, that genericists do *not* treat “the class of generics” homogeneously if by that class is meant, as Thompson implied was meant, something so large as also to include statistical generalisations—an advocate of the present, modal approach can respond further: even genuine generics are in *one* sense a “rag-bag”: *viz.*, they rest on different accessibility and ordering relations. But in another perfectly intelligible sense, they share a logical form: namely, the one revealed when we give their truth conditions “relative to world, accessibility relation and ordering source”, without specifying what the particular accessibility relation and ordering source for a given sentence are. (Something like this approach is taken by Papafragou (1996), and indeed also Heim, whose treatment Krifka *et al.* are for the most part following.) Thus, when Thompson argues that natural-historical judgments are logically distinctive even from the

(in some respects similar) class of judgments about artefacts and techniques, a modalist can perhaps attempt to reply: what distinguishes them is the difference in the *content* of the modal apparatus needed to evaluate their truth: they share the logical *form* of modalised conditionals. (Thus Thompson points out that the teleological order of technical judgments exhibits a “partial idealism” (in Anscombe’s sense (1976)), in that the truth of an articulation of that order in the case of a given technique “presupposes that someone makes or has made the corresponding judgment, or at least some others belonging to the same system of judgments.... An unrecognized technique is after all a merely possible one” (Thompson 1995: 294), whereas the truth of an analogous natural-historical judgment stands in no such need of having been recognised. But the modal order, it might seem, could be selected to accommodate this distinction: at a minimum, perhaps, by (for technical judgments) allowing as accessible to *w* only worlds in which no techniques exist that have not been developed in *w*.)

But *can* we find an appropriate modal base and ordering source for natural-historical judgments? Krifka *et al.* themselves, as we saw, raise what appear to be quite damning objections to the idea that a coherent account can be devised, illustrated by ‘A turtle is long-lived’ and ‘A pheasant lays speckled eggs’. Again, the truth conditions they give for generics are such as to make ‘A pheasant lays speckled eggs’ true in *w* just in case *any* pheasant in any world accessible to *w* is such that it lays speckled eggs in any most normal world. But then it is implied that, in any “most normal world”, every pheasant lays speckled eggs—hardly a biologically realistic conception of normality! In fact we face here many of the same problems that arose with respect to the ducks we considered in connection with the “prototype” approach to generic sentences. Thus (analogous to a problem I raised there) consider the sentence ‘Ring-necked pheasants have colourful plumage and lay olive-buff eggs’: the proposal requires that its truth condition be that,

in every “most normal world”, every pheasant⁴⁷ have colourful plumage and lay olive-buff eggs—although what is in fact normal for ring-necked pheasants is that the males be colourful and the females—when fertilised and so on—lay olive-buff eggs, not that a single bird instantiate both of these properties.

It may even be that these problems are more acute for the modal approach than for the prototype approach, because whereas there we envisioned (going beyond anything actually said by the discussants of the prototype approach, actually) generating a set of prototypical creatures for a given life form and taking a generic sentence to quantify only over those, here we are still quantifying over *every* member of a kind (at least, every member present in any world accessible to the world of evaluation). *This very pheasant*—the one standing next to the water cooler in world *w'*—has got to be both colourful and egg-laying in every most normal world (as does every other pheasant in every world accessible to this one).

§2.10. Situations.

Krifka *et al.* then consider, without too much detail or any criticism, the possibility of explicating generic sentences within the framework of situation semantics (Barwise and Perry, 1983). The advantage to shifting from quantification over possible worlds to quantification over situations is that, since situations needn't be *total* in the sense in which possible worlds are, we can avoid some of the problems arising for attempts to treat generics in possible-worlds frameworks: for example, we can consider a situation in which a pheasant is laying eggs, without supposing that the situation exhausts the world (and hence that there are no males to have fertilised her). More formally, the situation-semantic approach to generic sentences follows the situation-semantic

⁴⁷ strictly, again, every pheasant also present in a world accessible to the one at which the truth of the sentence is being evaluated

approach to conditional sentences, which is to say that it models them as constraints on situations. The basic idea of a constraint in this context is as a relation between situation types $\Sigma \Rightarrow \Sigma'$, to the effect that whenever a situation of type Σ obtains, so does one of type Σ' . The effect of quantificational variables is captured by the idea of anchoring situation types in parameters, such that constraints persist through the anchoring of the situation types involved in them: so that if we have $\Sigma \Rightarrow \Sigma'$, we also have $\Sigma(f) \Rightarrow \Sigma'(f)$, where $\Sigma(f)$ is just like Σ but anchored in some range of parameters. In addition, situation semantics expresses the idea that such a constraint only obtains against some *background* by expanding the two-place relation between situation types to a three-place relation among situation types and a background: we may write $(\Sigma \Rightarrow \Sigma')|B$ or $\Sigma \Rightarrow_B \Sigma'$.⁴⁸ Now we can express the situation-semantic model of generic sentences. As Krifka *et al.* put it, a generic sentence with its **Restrictor** and **Matrix**

is true relative to a background B in which [the variables bound by **GEN**], and possibly others, occur as parameters... iff:
 There is an anchor f for the parameters in B such that for every situation σ which is of type $B(f)$ it holds that if **Restrictor**(f) is true, then f can be extended to f' such that **Matrix**(f') is true. (58)

And they illustrate with ‘A pheasant lays speckled eggs’:

GEN[$x, s; y$](x is a pheasant in s ; x lays y & y are speckled eggs in s) is true with respect to the background ‘ s is a situation of giving birth’ iff:
 For every situation σ which is a situation of giving birth it holds that for any x which is a pheasant in σ , there is a y which are speckled eggs, and x lays y in σ . (58)

However, though this does appear to sidestep some of the objections to modal approaches we saw above, the formalism presumes that the background (that which is represented by ‘ B ’) against which we are to evaluate the truth of a given generic sentence is *fixed* for that sentence. My complaint is not that the formalism doesn’t *provide* the (content of) B —that would be an

⁴⁸ The original presentation of situation semantics, Barwise and Perry (1983), accommodated the idea of a background to a constraint by building it into the first place of the two-place relation: $(\Sigma \cup B) \Rightarrow \Sigma'$. However, later works in the tradition, I gather, have tended to prefer the approach described in the text.

unreasonable complaint, since the generic doesn't explicitly provide it either—but that the formalism, again, presumes that it is *fixed*. To see what I mean, first consider Krifka *et al.*'s example treatment of 'A pheasant lays speckled eggs' quoted above. (I'll pretend that this sentence expresses a true natural-historical judgment.) It may be granted that if a pheasant is "giving birth" (as they put it) in a given situation, it will be laying eggs. (Indeed, even *this* is granting too much: we can imagine a DDT-like substance in the environment preventing pheasants' eggshells from forming, so that pheasants in situations of "giving birth" might actually give birth!) But will they be speckled? Surely it sometimes happens that a pheasant lays eggs of an anomalous colour (perhaps due to dietary oddities or some such). The analysed truth condition they give for the sentence is, as we see, "For every situation σ which is a situation of giving birth it holds that for any x which is a pheasant in σ , there is a y which are speckled eggs, and x lays y in σ ." But the considerations just adduced show that this is false as it stands. Now, presumably (though Krifka *et al.* do not appear to have registered this fully) the background parameter B is meant to take care of this sort of thing—but it should be clear that this will only work if B can rule out *all possible* anomalies. However, it's not just that it's "hard to spell out" the background conditions the obtaining of which would guarantee the universality of the connection between restrictor and matrix (here, between situation types): it's that there *is* no fixed set of such. 'A pheasant lays speckled eggs', but an indefinite number of things could intervene: something in its diet could alter the eggs' colour, or the pheasant could be shot just before the eggs emerge, or some industrial pollutant preventing calcium absorption could have entered the pheasant's system, or a meteorite could strike the area, or.... There is no reason to suppose that there is a fixed list of such possible grounds for exception, even in principle. Hence

to model generics as though there were—as though the value of B is fixed once Σ and Σ' are given—is to be false to their semantics.⁴⁹

Additionally and more generally, the situation-semantic approach to generic sentences attracts the same objection I outlined at the end of §2.1 on Bacon. Namely, by isolating, however cleverly, the pheasants (to continue with Krifka *et al.*'s example) that are “giving birth”, and restricting the generalisation about speckled eggs to them, this approach obscures the “unity” attaching to the system of natural-historical judgments collectively describing a life form (say, the ring-necked pheasant), through which each individual judgment in some sense has application to *all* those exhibiting the form in question, even when they do not instantiate the particular judgment's matrix.

§2.11. Nonmonotonic inference.

The basic idea of nonmonotonic inference is that, in certain domains, it is useful to systematise a notion of inference for which the structural rule of Weakening is inadmissible: that is, a notion according to which it does not hold in general that when a conclusion A follows from a set of premises Γ , A follows from any superset of Γ . Though most scholars of nonmonotonic inference develop such systems with applications in artificial intelligence in mind, Krifka *et al.* point out

⁴⁹ In a similar context, Thompson considers the idea that generic sentences can be understood as universal generalisations qualified by tacit “*ceteris paribus*” clauses. He argues that to spell out these clauses for a given natural-historical judgment would itself require an appeal to the life form that is the subject of the judgment, and would thus not allow us to “escape the circle” that judgments involving life constitute. (Thus if the tacit condition behind ‘The Serbian mayfly breeds in mid-June’ is “barring outside intervention” or “supposing conditions are normal”, then ‘intervention’ and ‘normal’ must themselves be understood in terms of the natural history of the mayfly.) But this objection is not quite enough for us; for the formal semanticist wants merely to *model* natural-historical judgments, and as long as there *is* some background condition B—even if it is (quite explicitly recognised as) a function of the life form and predicate (the restrictor and matrix) in question, rather than something specifiable without reference to that life form—we do not have an objection to modeling such judgments as, say, $(\Sigma \Rightarrow \Sigma')|B$. My point is, rather, that there is no such B to be denoted, even by explicit reference to the life form in a locution such as “conditions being normal for the mayfly”. The problem is not simply that we cannot say what those conditions are except by reference to the mayfly, and hence that we cannot “break into the circle”, that is, reduce it to something outside the circle; it is that there is nothing for such locutions, circular or not, to denote.

that the application of the notion to generic sentences ought to seem promising, since the sorts of example that arise in the literature on nonmonotonic inference often involve generics. The standard example runs as follows: from ‘Tweety is a bird’, it is a good inference to infer ‘Tweety flies’; but from ‘Tweety is a bird’ together with ‘Tweety is a penguin’, it is no longer a good inference to draw that same conclusion.

There are, of course, treatments in the literature of nonmonotonic logic in abstraction from its context (Restall (2000) gives a nice introduction to substructural logics generally); there are also, as I have said, treatments of nonmonotonic logic with one eye on applications in artificial intelligence (Moore (1995) is an example, though his (1984, 1985, 1993) are not especially focused on AI). For the purposes of this chapter, I shall focus my attention on Asher and Morreau (1995), who explicitly develop an application of nonmonotonic logic to generic sentences.

Some treatments of nonmonotonic logic understand the sentences from which nonmonotonic reasoning is appropriate as *inference rules*: that is, rather than treating them as sentences on a par with other sentences of the object language, they give them a metalinguistic interpretation. Thus “default reasoning” approaches treat ‘The S is P’ as, roughly, “If ‘a is an S’ is true, and if ‘a is a P’ can be consistently assumed, then conclude that ‘a is a P’ is true”. But Asher and Morreau point out that this makes it very difficult to understand how such sentences can embed in larger sentential contexts; and Krifka *et al.* argue that this metalinguistic approach seems to run contrary to the thought (to which we have already made much reference in this chapter) that natural-historical judgments have a “transparently ‘factual’ or ‘positive’ character” (in Thompson’s words). Likewise, many treatments of nonmonotonic logic—which is after all an active research area in artificial intelligence—interpret generics epistemically, in terms of how

new information should be used to revise information states (cf. Engelfriet and Treur (1998) for one example). The attempt to apply this sort of approach to nonmonotonic logic to generics seems to fall to the same two objections.

Asher and Morreau devise an interesting treatment of generic sentences, in so far as they begin by acknowledging that such sentences enter into few patterns of deductive reasoning, but then suggest that, rather than therefore despairing of giving them a formal treatment, we ought to canvass as well the patterns of *nondeductive*—defeasible—reasoning, to see if we can come up with a semantics that can explain their inferential behaviour thus broadly construed. They produce a truth-conditional account of the semantics of generics (and hence sidestep the objections discussed in the previous paragraph), a variety of modal approach, and then they define a notion of defeasible consequence and show how their semantics for generics explains not only how generics figure in deductive reasoning but also why they behave the way they do in defeasible reasoning.

(By the way, Moore (1985) complains about McDermott and Doyle’s treatment of nonmonotonic logic that their semantics would render the nonmonotonic reasoning they attempt to formalise deductively *valid*, even though they remark informally (in Moore’s words):

... that their notion of nonmonotonic inference is *not* to be taken as a form of valid inference. If this is the case, their formal semantics cannot be regarded as the “real” semantics of their nonmonotonic logic. At best, it would provide the conditions that *would* have to hold for the inferences to be valid, but this leaves unanswered the question of what formulas of nonmonotonic logic actually *mean*. (126n1; Moore’s emphasis)

Plainly, Asher and Morreau’s account is not susceptible to this objection.)

To explain why I am nevertheless dissatisfied with Asher and Morreau’s account as applied to natural-historical judgments, I shall spare the reader a lengthy exegesis of their apparatus and cut

directly to the truth definition their system yields for its representation of sentences of the form ‘ ϕ s are normally ψ s’:

[I]t is true [at w] just in case for every individual δ , if we look at the worlds where $\phi\delta$ holds along with everything else which, in w , is normally the case where $\phi\delta$ holds, we find that $\psi\delta$ holds. (313)

One oddity of this account is that it indexes “normality” to the *individual*. If I’m not mistaken, we seem to be saying: ‘Birds fly’ is true at w iff, for any bird you like—say, Big Bird, an ostrich at the local zoo—the worlds where ‘Big Bird is a bird’ holds as well as everything that, *in w* , normally goes along with ‘Big Bird is a bird’ are all worlds where ‘Big Bird flies’ holds. In other words: “everything that normally goes along with” is *inside the scope of the quantifier*: it attaches to *each individual* predication, rather than to the quantification as a whole. So we have to consider what (in w) “normally goes along with” Big Bird’s being a bird (and *ditto* for all other birds). But what prevents us, then, from thinking—especially when w is the *actual* world—“Well, what (here in w) normally goes along with Big Bird’s being a bird is his being 2.5 metres tall, weighing 120 kg, being somewhat depressive, etc. etc.—and his *not flying*”?

Also, Asher and Morreau concede a criticism of Carlson’s, to the effect that their system doesn’t capture the inference (even as a defeasible one) from {‘Dogs are hairy’, ‘Dogs have four legs’, ‘Fido has three legs’} to ‘Fido is hairy’. (Since the second and third premises together vitiate the thought that Fido is “normal”, their system no longer countenances any of the defeasible inferences that presuppose that we do not know that Fido is not normal.) They gesture at a modification of their account, which they have not yet succeeded in seeing how to make, involving the introduction of a notion of “degrees of normalcy”, allowing one to consider worlds where Fido (*e.g.*) is anyway no *less* normal than one already knows. They propose (again, so far only in briefest outline) doing this in terms of the *number of predicates* normally associated (in

w) with doghood that Fido lacks. But it is hard to find promise in this attempt to save the account from Carlson's criticism. Surely in the example argument, what matters isn't that Fido is (so far as we know) only one predicate away from normality (!), but rather that *number of legs* just has *nothing to do* with hairiness. (If the third premise in the sample argument had been 'Fido's follicles are defective', it would seem as though their envisioned cardinality-driven system would have to treat it just as it would the sample argument itself: but clearly the conclusion in this case *isn't* equally well justified by the premises. (And it is in any case already a puzzle how to quantify the "number of predicates" at issue: Fido instantiates 'has three legs', but also (as we can infer without further information) 'has less than four legs', 'has an odd number of legs', 'has a prime number of legs', and so on. How many predicates away from normality is he?))

Finally, in so far as the semantic core of their proposal is a species of modal approach (albeit an ingenious one, especially in the way it is supplemented with a theory of defeasible reasoning), Asher and Morreau's treatment of generic sentences is threatened by the objections we've already canvassed to modal approaches. Thus, for instance, another criticism of Carlson's they attempt to address is a form of one we've seen already: because of the way in which their semantics for generic sentences involves universal quantification, it would seem to have the implication that, if 'Chickens lay eggs' is true, then so is 'Chickens are hens'. They respond that the cause of the difficulty here is a matter of determining the restrictor implicit in a given natural-language quantificational statement—a matter which is as pressing for standard universal quantifications as for any other types of quantification. (Thus—their examples—the chair of a committee meeting remarking 'Everyone is here' is not quantifying over the whole human population, and 'John always feeds the cat' is not quantifying over all moments of time; but

these two sentences are nevertheless correctly understood as involving universal quantification.) Determining the restrictor of a quantification, they go on, can be difficult (and they recommend drawing on Lewis's notion of accommodation (in his 1979) for help), but it is no special difficulty for generic sentences in particular. However, I'm not sure that specifying the restrictor as narrower than the natural-language sentence seems to present it will actually solve the problem; and I'm not sure that it really is precisely the same problem as in the nongeneric examples they give: I'm not sure it's not after all a special problem for generics. The implication is that an utterance of 'Chickens lay eggs' could quite naturally be followed with 'I mean the female ones, of course' (just as 'Everyone is here' could be followed with 'I mean, everyone on the committee'). But (again) consider an utterance of 'Ring-necked pheasants have colourful plumage and lay olive-buff eggs': with what could we follow this so as to specify a restriction on the antecedent concept to yield a truth? Neither 'I mean the female ones' nor 'I mean the male ones' would do. Should we understand the sentence as short for 'Ring-necked pheasants have colourful plumage and ring-necked pheasants lay olive-buff eggs', and then imagine it supplemented as 'Ring-necked pheasants have colourful plumage—I mean the male ones—and ring-necked pheasants lay olive-buff eggs—I mean the female ones'? (That is, should each conjunct be run through Asher and Morreau's semantic machinery separately, each time presuming a (separate) antecedent accommodation of the restrictor of the quantification?) But this seems false to the information an utterance of the original sentence would convey. It may well be that anyone with the most rudimentary knowledge of biology could supply for herself the qualification on 'Ring-necked pheasants lay olive-buff eggs'; but it is less widely known that it is only the males that are colourfully feathered, and in any case it is clearly implausible that this fact is *communicated* by the sentence 'Ring-necked pheasants have colourful plumage and lay

olive-buff eggs’. One can learn from this sentence that the two properties in question are characteristic of pheasants, without learning or even assuming anything about their respective distributions among the individual pheasants.

§2.12. “Distributive” approaches: summary of results.

I have canvassed a wide range of broadly “distributive” approaches to the semantics of generic sentences, and found them all wanting. I suggest that this is no coincidence: for, firstly, as Gupta and Savion pointed out (and as we quoted earlier), this sort of approach, namely “of taking generic sentences to be implicitly quantificational, can survive refutation only by being evasive on the nature of quantification involved” (1985: 859). The purpose of the preceding section was to press this objection in the face of the existence of some less evasive proposals; the result has been, as expected, that, when less evasive, they cannot survive refutation: no quantificational approach satisfactorily captures the force of generic sentences.

“Distributive” approaches have in common that they interpret the restrictor and the matrix of a generic sentence—in our particular case, the “life form word” and the predicate ascribed to it in an Aristotelian categorical or other expression of a natural-historical judgment—both as (perhaps complex) *predicates*, true or false of individuals (and in our particular case, of individual organisms). In the simplest case, they interpret generics as giving the *quantity* of restrictor-satisfiers satisfying the matrix; but Thompson points out (as do many scholars of generics) that there is no such quantity either necessary or sufficient to guarantee the truth of the judgments in question. Slightly more sophisticated approaches attempt to understand generics as restricting the domain of restrictor-satisfiers before making the quantificational (now usually universal) claim; but we have seen that no subset of restrictor-satisfiers need be guaranteed to satisfy the

matrices of all true generic sentences with the same restrictor. (And it doesn't help to recognise that the function from the restrictor to the subset in question must be intensional.) A further refinement is the proposal, in the face of this, that we understand a generic as involving a *two*-place (perhaps intensional) function of *restrictor and matrix*, yielding as value a subset of the restrictor-satisfiers, and then an (again usually universal) quantification attributing to the members of this subset satisfaction of the matrix. But we found even in this case that no such function applied consistently to the whole range of natural-historical judgments would yield truths in all and only the right cases. Other approaches, perhaps a bit further from the “distributive” paradigm, take generics to predicate the matrix not of the members of any subset of actual restrictor-satisfiers, but rather of some ideally constructed restrictor-satisfiers—prototypes, stereotypes⁵⁰, and so on. But we saw fatal problems with the idea of this construction—whether, again, the construction of such ideal entities is conceived as a function taking the restrictor alone as argument or as a function of the restrictor and matrix together. We also considered proposals, still within the generally “distributive” framework, that attempt to capture what one might want to call the “normative force” of natural-historical judgments using modal apparatus, quantifying over possible worlds or situations (and perhaps erecting a system of nonmonotonic inference on this modal foundation). Although the modal apparatus may do a better job of homing in on the individuals witnessing the truth (as it were) of a given natural-historical judgment, however, we saw that it raises problems of its own; and in any case, such approaches persist in what I have been urging to be the fundamental error of distributive approaches, namely, the attempt to construct natural-historical judgments out of such a collection of predications of (now actual or possible) individuals. My claim is that the fundamental reason

⁵⁰ Actually the “stereotype” approach didn't take quite this specific a form; but, for lack of formal-semantic details, we can say roughly that the idea was that a generic sentence is true iff its matrix is true of “our stereotype of” its restrictor. The remarks in the text are meant to cover this (admittedly vague) conception.

for which all of these proposals fail is that the truth of a natural-historical judgment is simply not correctly conceived of as a matter of the satisfaction of the predicate of the judgment by any set of actual or possible individuals of the life form of which it is predicated: it is not a functional combination, of that sort, of the restrictor and the matrix.

There can be no doubt that, at the linguistic level, it must be harmless to view a generic sentence as the value of a certain function taking the restrictor and the matrix as arguments. ('The domestic cat has four legs' is just that sentence which combines '___ is a domestic cat' and '___ has four legs' (taken in that order) generically, if you like.) But what appears to emerge from our enquiry is that, at least as far as natural-historical judgments are concerned, any attempt to specify the *Bedeutung* of this linguistic function as a function from pairs of first-level functions into {T, F}, or indeed on any other function-argument model of the combination of predicates, comes to grief.

§3: Subject-predicate approaches.

In view of the problems associated with trying to unpack generic sentences in terms of quantification (however refinedly), some theorists argue that generics—or at any rate some of them—are best understood simply as singular predications of kinds. By a “subject-predicate approach” I shall mean one like this: one which treats (at least some subclass of) generics as singular predications, rather than analysing the noun phrase in subject position into a quantifier phrase or in some other way. Thus Carlson’s early work (1977, 1982) contains an argument for treating generics this way, motivated in the first instance syntactically, and supplemented by the claim that “generic terms” cannot be understood as sets, or as power sets or as intensions of sets, but must simply be treated as basic entities of the model, in the domain together with individuals. Krifka *et al.* (1995) (though we have seen them discuss “distributive” approaches at length) end

up advocating a subject-predicate interpretation of a wide range of generic sentences. E.J. Lowe (1991) practically takes for granted that generics can't be understood quantificationally (even in terms of restricted quantification or plural quantification); he gives a quick Quine-inspired argument for the claim that the noun phrases in question refer, on the grounds that we can treat them syntactically as follows: "What does Fido chase? —Cats." (To be is to be the value of a variable.) (He also adduces some additional syntactic arguments.) Lowe calls such phrases singular sortal referring expressions, and distinguishes them from names of species, arguing that, for example, 'Fido chases cats' doesn't mean that Fido chases the species (where by 'the cat species' Lowe understands the mereological sum of cats).

However, there are some major obstacles facing "subject-predicate" approaches as well. Let us start with Gupta and Savion again, who raise serious questions for Carlson's interpretation of "generic terms" as making singular reference to kinds, which he understands as "basic entities" on a par with individuals. For one thing, Gupta and Savion observe, {Dogs live in the northern hemisphere, No dog ever has lived or ever will live in the northern hemisphere} seems like an inconsistent set, but Carlson cannot account for its inconsistency. Further, without "an unusual Meinongian account of kinds, his theory will turn contraries into contradictories": for instance, to adapt an example of Gupta and Savion's to a natural-historical case, it could be that 'Mayflies breed in the afternoon' and 'Mayflies do not breed in the afternoon' could (construed as natural-historical judgments) both be false—if, say, they breed indifferently throughout the day; but if 'Mayflies' denotes an entity, these become contradictories. In short, Gupta and Savion insist, we need an *account of kinds* before we can judge the theory.

We can also point out (what in fact moved Cohen to his "type-shifting" interpretation of "inductivist" bare plural generic sentences) that interpreting generics in this subject-predicate

fashion often yields puzzling results. For instance, Carlson hears ‘The tiger is striped’ as a singular predication of the kind *tiger*, but (as we saw in our discussion of Fara) it seems quite odd to understand the sentence as expressing the thought that the tiger kind is striped. It is not even clear what that would mean. One’s *prima facie* inclination is to find ‘is striped’, and indeed also ‘tiger’, appearing univocally in ‘Tigers are striped’ and ‘Tigger is a tiger and he is striped’. But combining this inclination with the subject-predicate approach yields the result that ‘striped’ must be something predicable at once of material objects such as individual tigers and of kinds.

Several critics also point out that, despite the claims for syntactic parallels between “generic terms” and singular terms, there are phenomena such as scope ambiguity that attach to “generic terms” but not to genuine singular terms, as we saw in several of the discussions of “distributive” approaches in §2, and indeed as is at work in Gupta and Savion’s point about contraries and contradictories.

I hope to treat Lowe’s account at some length in another work, since his treatment of generic sentences in *Kinds of being* at once is sympathetic to some of our concerns and threatens a possible serious counterexample to our claim. For Lowe gives a kind of proof theory for a formalism in which he represents kinds and individuals distinctly, and also dispositional and occurrent predication distinctly—indeed his system purports to capture, what I am concerned to argue against the possibility of capturing in a formal system, two modes of predication involving the very same elements (that is, the same predicate and the same object of predication). (Further, he is sensitive to the problems with “distributive” approaches, and also to the difficulties for “subject-predicate” approaches such as those involving scope ambiguity.) I believe, though, that the absence of a formal semantics for this system is revealing, and indeed that his informal “consistency proof”, which reinterprets his system as a standard presentation of predicate logic

(thereby showing that it is consistent if the latter is), actually shows his two forms of predication to be a kind of sham, coming to nothing more than a partition on the set of predicates in the language.

§4. Conclusion: The structure of “natural-historical judgments” as nonfunctional.

I said already in §2.1 above that any “distributive” approach risked falling to the objection that, by attempting to interpret natural-historical judgments as distributions of the property expressed in the matrix to *some subset* of instances of the restrictor (in order to accommodate the phenomenon of exceptions), it fails to recognise the *unity* of the life form: that is, it fails to recognise the sense in which (to put the point a little metaphorically) a natural-historical judgment “speaks of” *all* instances of the kind of organism that is its topic. I also alluded above (in footnote 23 to §2.3, on Cohen’s adaptation of Carlson’s distinction between “inductivist” and “rules-and-regulations” approaches to generic sentences) to the inclination some scholars find, surely upon contemplating the same phenomenon of exceptions, to ascribe to natural-historical judgments (if not to generic sentences generally) some sort of *normative* or *regulative* character. Both of these considerations fuel the attraction of “subject-predicate” approaches. For, first, to understand a natural-historical judgment as a singular predication of a kind is, palpably, to remain true to the “unity” of that kind; and second, if after all “the tiger” (say) is striped, then surely—one wants to say—any particular tiger that isn’t striped “falls short of” the standard set for it by its kind in that respect.

But the considerations we just (briefly) canvassed in opposition to “subject-predicate” approaches suggest that to succumb to this attraction is to let the pendulum swing too far back. To put the point in the form of a slogan, it is to ascribe *too much* unity to the life form. Natural-historical judgments turn out to constitute a battery of descriptions of some particular “basic

entity”, whose connection to individual instances of the form becomes obscure. If we posit (as proponents of this sort of approach inevitably do) a relation of “instantiation” holding between individuals and kinds, we will nevertheless have nothing to say about the predicates which can be employed both in natural-historical judgments and in their instances, such as ζ *is striped*.⁵¹ What it is for such a predicate to hold of an individual is surely different from what it is for it to “hold of a kind”; but such systems do not illuminate this distinction.

But the function-argument analysis of discourse presses us to make a choice between these two types of approach. If we parse the “subject term” of a natural-historical judgment as, indeed, having the semantic value of a singular term, we will not at once be able to understand it as having the semantic value of a predicate; and conversely. (The point will hold whatever functional account we choose to give for predicates and for singular terms, for they will surely, at any rate, be different.)

In short, the general moral to extract from our discussion is that *function-argument analysis*, which is intimately related at once (and not coincidentally) both to the Fregean conception of a *begriffsschrift* and to the sort of formal-semantic endeavour we have been examining, cannot cast light on the natural-historical judgment. If we understand “life form words”, and the predicates ascribed to them, simply as elements which combine with individual terms to yield

⁵¹ By “have nothing to say” I do not mean that no advocate of a “subject-predicate” approach has so much as noticed the matter. On the contrary, Carlson (1977) proposed (as have others) introducing an operator on predicates which turns a predicate suited to apply into an individual into one suited to apply to a kind: thus ‘Tigger is striped’ might be represented as *Striped(Tigger)* and ‘The tiger is striped’ as *G'(Striped)(the tiger)* (I am abstracting from the details of his treatment of the noun phrase ‘the tiger’). But Carlson gives us very few details about the interpretation of the operator *G'* in turn: only a couple of minimum constraints on how it must behave, rather than a substantive proposal. —In fact, if we had a coherent account of *G'*, we might well have a response to Gupta and Savion’s complaint about turning contraries into contradictories: for (in our earlier example) ‘Mayflies breed in the afternoon’ and ‘Mayflies do not breed in the afternoon’ would be represented respectively as *G'(breed-in-the-afternoon)(the mayfly)* and *G'(~breed-in-the-afternoon)(the mayfly)*, and these are not contradictories. But absent an account, *G'* merely labels the problem rather than solving it. (Indeed, absent an account, we have no reason to understand, say, *G'(Striped)* as having anything to do with *being striped*.)

predications, we shall not be able to cobble together (“distributively”) a natural-historical judgment from such materials; but if we think of “life form words” simply as denoting basic entities, the life forms, we shall not at once be able to respect the univocality of their uses in generics and the predicative use to which they can also be put.

But we learned in the previous chapter that the idea of a *begriffsschrift* (with its eminent suitability for function-argument analysis) is essential to Frege’s very conception of the logical. If natural-historical judgments cannot be captured in such a system, then the claim that they exhibit a unique logical form is at once a claim that Frege’s conception of logical form is unsatisfactory. Thompson casually refers to a “properly *begriffsschriftliche* formulation” in which it would be revealed that sentences such as ‘Some animals are viviparous’ involve quantification over life forms (1995: 283). This betrays his insufficient recognition of the degree to which he is departing from Frege. To portray the difference between Thompson’s Wittgensteinian conception and Frege’s as simply a matter of the *number* of distinctions in logical category drawn (cf. Thompson, 1995: 249n4), and to suggest that an updated *begriffsschrift* could accommodate this wider range of distinctions, misses the fact that with Frege’s commitment to formalisability comes a principled limitation on the range of logical categories.

But if natural-historical judgments cannot be construed as functional combinations of elements, how are we to understand their composition? And what are we to make of the status of the post-Fregean conception of the study of logic as having at its centre the construction of *begriffsschriften* of Frege’s sort? Specifically, do we have any reason for understanding the mode of combination of elements in natural-historical judgments, which I have argued is not

functional, as a matter of their *logical form*, rather than, say, a matter of the peculiarity of their content?

I shall approach this question historically, by spelling out a hint in a footnote of Thompson's, who (as we saw at the beginning of this chapter) suggests that we ought to understand his claim about the logical uniqueness of natural-historical judgments in terms of Wittgenstein's broadening of Frege's conception of logical categories. In Chapter V, I will attempt to show how Wittgenstein's engagement with Frege's and Russell's views of logic that led to the *Tractatus* then paved the way for a shift in the conception of propositional articulation away from the functional structure necessary for formalisability and toward a richer conception, that can be perceived to be at work in the *Investigations*, outstripping the limits of formalisation. But before that, in Chapter IV, I will explain why I am not ready to accept Sebastian Rödl's claim that the conception of logical form at issue in Thompson's work and indeed in the *Investigations* is just Kant's conception of transcendental logic.

Chapter Four. Is Wittgenstein's *Philosophical investigations* in need of transcendental vindication?

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7. Is the *Investigations* in need of a transcendental vindication?

1. Is the *Investigations* in need of a transcendental vindication?

Sebastian Rödl, in his *Kategorien des Zeitlichen* (2005), claims that the later Wittgenstein in his “grammatical” investigations, and scholars such as Gilbert Ryle, Elizabeth Anscombe, John McDowell and Michael Thompson in their employment, in one context or another, of a notion of *form* influenced by Wittgenstein, are engaging—unawares—in investigations into transcendental logic in Kant's sense. He claims that, on the one hand, “‘logic’ in the analytic tradition now only designates the science of formal calculi” (2005: 8); an implication of this is that, if a notion of “logical form” is operative in that tradition, it will depend on the location of judgments in a deductive order of the sort limned by such a calculus. This notion of logical form (which Rödl also allies with what Kant called “general logic”: *ibid.*, pp. 13-14) has roots in Frege's work:

indeed, “Frege’s central thought was that a certain deductive order characterizes thought as such” (*ibid.*, p. 8)—though Rödl holds that the analytic tradition has retained Frege’s emphasis on deductive order while dropping the notion of the characterisation of “thought as such,” with the result that the application of a formal calculus to discourse “can only be pragmatically justified” (*ibid.*, p. 9). Meanwhile, however, the notion of form operative in the investigations of Wittgenstein and his intellectual heirs cannot be the Fregean one, since these authors simply do not deal with questions of the systematisation of inference. Rather, their notion of form can only be understood as “logical” in the transcendental sense. Rödl claims that the corpus of work in the Wittgensteinian tradition thus stands on shaky foundations as long as the principle from which its formal categories spring has not been recognised and vindicated, in something like the manner in which Kant provided a “transcendental deduction” of the objective validity of his categories. Without such a grounding, “an obscurity is cast over that strand of the analytic tradition which arises from the *Philosophical investigations*, an obscurity over the nature and possibility of ‘grammar’ and thus over the object of philosophical investigation” (*ibid.*, p. 10).

Now, Rödl’s is not a work on Wittgenstein: his project is to fill out and defend the transcendental-logical conception of form as contrasted with the Fregean conception, and in particular to bring out the centrality of *temporality* for the former; the remarks on Wittgenstein to which I’ve just alluded are relatively brief and programmatic. But these remarks nevertheless implicate Wittgenstein deeply in the story Rödl tells: he writes that “Frege’s central thought” about the connection between deduction and thought “has an overpowering effect on... Wittgenstein’s *Tractatus*”, which work however “at the same time leads the Fregean thought... to the point of its dissolution” (*ibid.*, pp. 8-9). Rödl’s idea is that, with the failure of the *Tractatus*, Wittgenstein alters his own conception of logic radically, in effect to the

transcendental-logical conception, in his later work. And the exemplars of the transcendental-logical conception of form in contemporary analytic philosophy Rödl holds up are, in one way or another, influenced by (the later) Wittgenstein. If this is so, then surely a closer examination of Wittgenstein's rôle in this story, and of the aptitude of Rödl's attributions to the earlier and later Wittgenstein of these distinct conceptions of form, is worth pursuing.

And further, this story obviously bears a close relation to the one in whose telling the present work consists. I have argued in Chapter II that central to Frege's conception of a *begriffsschrift* is interpretability in function-argument terms; and I have been taking it for granted that the idea of a *begriffsschrift* in turn is central to Frege's conception of logic. Following Thompson—one of the exemplars of the tradition Rödl holds up in contrast with Frege's approach to logic—I have argued in Chapter III that natural-historical judgments are not amenable to function-argument analysis, and hence are not susceptible of regimentation in a Fregean *begriffsschrift*. I suggested at the end of that chapter that an implication of these results was that Thompson's conception of logical form must be further from Frege's than Thompson recognises, given his references to the “properly *begriffsschriftliche* formulation” of certain judgments involving life form words, as well as his emphasis on the claim that his distinction between life form and vital operation is a distinction “of the sort Frege introduced” (1995: 283, 249n4; emphasis removed). (Thompson's meaning, of course, is that the distinction between life form and vital operation is a distinction in the categorial structure of thought on the same order as Frege's between object and concept; and nothing I have said casts any doubt on this. What I have been emphasising is the relation Frege sees between such categorial distinctions and the regimentation of inference—as we saw Rödl put it, “Frege's central thought was that a certain deductive order characterizes thought as such”—and so, since Thompson's method, in demonstrating the categorial status of

his favoured concepts, is not the regimentation of inference, his conception of the nature of these categories must be in this respect (and that is to say, essentially) different from Frege's.)

This is all to bring out the close affinity Rödl's account has to what I have been doing. But I am not convinced that his positive account of the conception of logic at issue in Thompson's work is quite right. I am not convinced, in particular, by the suggestion that Wittgenstein's conception of grammar (which Thompson indeed appears to consider akin to his own investigations: cf. again his p. 249n4) is in effect one of transcendental logic, and hence in need of foundation in a principle. In this chapter, then, I shall try to show why I think Wittgenstein's grammatical investigations do not stand in need of a foundational justification. In the following chapter, I will describe how the *Investigations*'s conception of grammar arose out of reflective engagement with, rather than utter repudiation of, conceptions of logic at issue in Frege's and Russell's work; in this way I will attempt to show that there is another option beyond Rödl's stark dichotomy between the "Fregean" and the transcendental conceptions of logic and logical form.

Let me begin, then with an examination of Wittgenstein's rôle in Rödl's story.

2. Two readings of the *Investigations*

Even before embarking on this examination, however, Rödl's particular claims about the place of the later work of Wittgenstein and of his intellectual heirs seem open to grave *prima facie* doubts. For the idea that a thorough understanding of the significance of that work "requires developing a system of grammar from a principle"¹ seems manifestly to run counter to Wittgenstein's own developed views of the scope and proper ambitions of philosophy. At the very least, it seems clear that Wittgenstein does not think of his grammatical investigations as awaiting a foundation,

¹ The phrasing is Rödl's, from personal communication.

of the sort provided by a “principle”, not itself expressed in the *Investigations*: the “grammatical remarks” made there seem to be presented as self-contained. This might incline us to think that whatever foundation or principle is needed is already there in the book. But further, it seems equally clear that Wittgenstein thinks of the search for foundations of this sort as entirely misguided: that it is not that his grammatical remarks sit on a firm foundation, nor much less that they are awaiting a foundation still to be provided, but that there is simply no such foundation to be had. There is no “principle” from which to develop a “system” of grammar: there is simply grammar.

To cast these *prima facie* impressions in terms closer to those which Rödl uses, we might say: if, on the one hand, it is right to think of Wittgenstein as giving something like a “transcendental logic”, he at any rate does not think of his work as standing in need of a further vindication of the objective validity of grammatical categories, but rather as providing already whatever vindication is to be given; but on the other hand, it may on the whole be better to think of his work—or of his own understanding of it at any rate—not as providing a “transcendental logic” (or grammar) whose categories have been or are to be vindicated at all, but as doing something else entirely.

Now, none of this will come as news to Rödl, who himself observes that Wittgenstein “appears to want to reject the very idea of a general form of thought,” not to mention the project of vindicating such an idea, but who is in any case claiming that the absence of such a vindicating account yields an “obscurity”, a lack which must be repaired (*ibid.*, pp. 10, 13). So merely appealing to Wittgenstein’s self-understanding will obviously not suffice to dislodge his work from the place in his story Rödl wishes to assign it. These *prima facie* observations put some pressure on Rödl’s view of the significance of the work, by showing the extent to which it rests

on that attribution to it of a (not merely insufficient but) false self-understanding. *Ceteris paribus*, a more charitable reading would make sense of the rest of the work in a way that preserves its self-understanding. But perhaps, in the end, Rödl's will prove to be the most charitable reading available. The only way to settle the issue is to dive into the details: to examine the grounds on which Wittgenstein rejects the project of vindication, and to determine whether these really can be reconciled with the pretensions behind the "grammatical remarks".

I suggested above that there are two different ways in which we might understand Wittgenstein's "rejection" of the project of vindication: he might hold that his work in fact already *contains* whatever kind of vindication could be hoped for, or (giving what is perhaps a more literal reading to some of Wittgenstein's methodological remarks) we might suppose that he really means that no "vindication" is necessary at all (nor is it possible). I will begin by addressing this first, preliminary question, which of these two readings of Wittgenstein's work—of the significance that Wittgenstein himself sees in it—is more plausible; indeed this will occupy us for most of the chapter. This will put me in a better position to make some remarks concerning the further issue, whether Rödl is right to find fault with Wittgenstein's own understanding of his work (whichever way we end up making that out), at the end of the chapter.²

3. An ill-posed question?

But before I launch into this, I need to allay one more worry which may have arisen in connection with how I've been setting out the dialectic. For it was implicit in Rödl's remarks discussed at the beginning of this chapter that when one purports to describe the structure of thought and talk, one must be doing either general or transcendental logic—but one might object

² After all, again, these two questions aren't quite as neatly separable as the text suggests: for the question how best to understand Wittgenstein's own conception of the import of his work is of course controlled to some degree by the

that the later Wittgenstein makes no claim to be interested in describing “the structure of thought and talk” at all. Thus Jonathan Lear claims that:

... whereas in the *Tractatus* it is hoped that we will... recover the structure of thought, by the time the *Investigations* was written, Wittgenstein realized that there is no interesting structure worthy of recovery. (1982: 385)

Now, there is no doubt that Lear is onto something here. But the point needs some care; for contrast some of Wittgenstein’s words from the very work to which Lear is referring:

This [*sc.* the idea that our “analysis” of language aims at an ideal of complete exactness, complete analysis] finds expression in questions as to the *essence* of language, of propositions, of thought.—For if we too in these investigations are trying to understand the essence of language—its function, its structure,—yet *this* is not what those questions have in view. For they see in the essence, not something that already lies open to view and that becomes surveyable by a rearrangement, but something that lies *beneath* the surface. Something that lies within, which we see when we look *into* the thing, and which an analysis digs out. (*Investigations*, §92)

Here, Wittgenstein himself, like Lear, draws a contrast between the approach of the *Investigations* and one plausibly attributed to the *Tractatus*. But Wittgenstein shows himself willing to associate the idea of *structure* with what he is after in the *Investigations*: only, a structure “that already lies open to view”. —Still, is this too thin to sustain the sort of question with which we began—namely, whether the sort of structure Wittgenstein is describing requires a transcendental foundation?³

Plausibly not. The context of the passage quoted (especially §90) makes clear that what Wittgenstein has in mind is *grammar*. This “lies open to view” in the sense that grammatical propositions are *obvious* to us (though the need for the grammatical investigation arises from our tendency to allow, for instance, “certain analogies between the forms of expression in different

question what such conception strikes us as correct. To that degree, we’ll be addressing the second question, as well as the first, from the beginning.

³ I do not mean to suggest that Lear himself takes his observation about a difference between the *Tractatus* and the *Investigations* to imply that the latter’s conception of structure *is* too thin to sustain such questions. Indeed, Lear’s articles will provide a paradigm of the sort of reading of Wittgenstein’s later work that has him not only engaged in transcendental philosophy (as Rödl suggests) but carrying it to completion (as Rödl denies).

regions of language” (§90) to lead us into confusion, to lead us to forget what we know about our grammar). But this already seems to be enough to sustain those questions: for we may well find ourselves asking what *warrants* our finding those grammatical propositions obvious (whether, say, they “correspond to anything in fact”). And this was just Rödl’s complaint: that if one is to appeal as Wittgenstein does to grammatical knowledge at all, one must show how synthetic knowledge *a priori* is possible (for Rödl holds that that is what Wittgensteinian “grammatical knowledge” is: *ibid.*, p. 9) by developing a system of grammar from a principle.⁴

I hope this is enough to justify setting aside, at least for now, the worry that the later Wittgenstein is simply not interested in “describing the structure of thought and talk” in any sense thick enough to make it look even worth asking whether such description requires some sort of transcendental grounding which Wittgenstein does or does not provide. In any case, the second proposed reading of Wittgenstein’s own intentions—namely, that he means not so much already to have given a sort of transcendental grounding to his grammatical reflections, as rather to have rejected the demand for such grounding altogether (a reading which, in any case, I shall recommend)—may come close to accommodating the worry. For one way of maintaining that his grammatical reflections stand in no need of vindication could be to insist that they simply don’t constitute the sort of system for which vindication (transcendental or otherwise) is in order.

4. The *Investigations* as transcendental logic: Williams and Lear

Let me therefore begin by setting out the case for the idea that the *Investigations* does provide a transcendental philosophy, and what’s more, contains its own version of a transcendental deduction of objective validity. In §5, I shall consider the reading according to which the nature

⁴ Note that I do not mean here to endorse Rödl’s characterisation of the content of Wittgenstein’s grammatical remarks as “synthetic knowledge *a priori*”; the question how best to conceive of their content is of course inseparable from the question what sort of grounding, if any, they require.

of Wittgenstein's grammatical investigations is far enough from transcendental philosophy not to require anything like such a deduction.

Bernard Williams suggests that there is room for a view according to which:

... *our* language, in [the] sense in which its being as it is has no empirical explanation, shows us everything as it appears to our interests, our concerns, our activities, though in the only sense in which we could meaningfully say that they determined everything, that statement would be false.... [T]hat provides grounds, I suggest, for calling such a view a kind of *idealism*.... (1974: 153; Williams's emphasis)

And he suggests—tentatively and with qualifications—that the work of the later Wittgenstein is pointing toward such a view, as his earlier work pointed to a first-person-singular predecessor of it:

[W]hile much is said by Wittgenstein about the meanings *we* understand being related to *our* practice, and so forth, that *we* turns out only superficially and sometimes to be one *we* as against others *in* the world, and thus the sort of *we* which has one practice as against others which are possible in the world.... [O]ne finds oneself with a *we* which is not one group rather than another in the world at all, but rather the plural descendant of that idealist *I* [of the *Tractatus*] who also was not one item rather than another in the world. (1974: 160; Williams's emphasis)

That is: On Williams's reading of the *Tractatus*, the shrinking of the 'I' which results in the coincidence of solipsism with pure realism, and which implies that, if there is any way in which "in philosophy we can talk of a non-psychological I" (§5.641), it must be "not with sense" (Williams 1974: 146), nevertheless leaves *something* "transcendental" to be gestured at: the 'I' is certainly not an empirical object in the world, nor can it even be spoken of sensibly as a *nonempirical* object (since nothing not in the world can be spoken of sensibly), but it (as "a limit of the world", §5.632) is still that which anchors the insight that "what solipsism *means*, is quite correct" (§5.62).

Wittgenstein's later work on privacy, continues Williams, serves to undermine solipsism and phenomenalism, but leaves room for a gesture at an updated idealism. Just as, in the *Tractatus*, for an 'I' not a part of the world, there is something right about the thought that "the limits of my

language mean the limits of my world” (§5.6), so a kind of plural form of idealism—one whose plurality insulates it from the privacy considerations—can gesture at the truth that “the limits of *our* language mean the limits of our world.” The ‘we’ in question must, like the ‘I’ of the *Tractatus*, not be one among many in the world (and likewise for the sense of ‘language’ at issue); but this is not untrue to the method of the *Investigations*. Though we are frequently invited to contemplate putative alternatives to our practices, we are meant not to take them seriously as genuine alternatives, but to use them as matter for deeper reflection on our own practices—the only ones we can genuinely understand.

Jonathan Lear picks up Williams’s suggestion of a gesture at transcendental idealism in Wittgenstein’s later work and runs with it. In “Leaving the world alone” (*op. cit.*), he uses the expression “being *minded* in a certain way” to speak about the sort of reflective exploration of our practices that Williams finds in the later Wittgenstein’s work: the nature of our mindedness is what that exploration facilitates our coming to grips with. But Lear, like Williams, insists that, for Wittgenstein, our mindedness is not one among many: “The possibility of there being persons who are minded in any way at all is the possibility of their being minded as we are” (1982: 386). To emphasise our mindedness is precisely *not* to imply that “we could have been minded otherwise”: we cannot make sense of this putative possibility. And (echoing Williams again: cf. his 1974: 154ff.) there can be no real *explanation* of our being minded as we are: for, again, this isn’t one empirical possibility among others. Still, Lear suggests that, even by Wittgenstein’s lights, there is *some* sort of insight to be captured along the lines of “Only because we are minded as we are do we see the world the way we do” (1982: 392) (though this does not support the inference to the counterfactual “If we were other-minded, we would see the world

differently”): and *this* is the sense in which, Lear claims, (the later) Wittgenstein is a transcendental idealist.

And in his contribution to the symposium on “The disappearing ‘we’”, Lear takes the attribution of transcendental idealism to the later Wittgenstein’s work, and the analogy that that attribution implies between it and Kant’s version of idealism, literally enough to find in the *Investigations* a “transcendental deduction” of the legitimacy of our ways of understanding. Kant, on Lear’s account, was concerned with the question how it is that thought and intuitive experience are in harmony; his response was to argue that we *constitute* this harmony—we constitute *experience*, as experience of *objects*—by uniting the manifold of intuition (in accordance with the categories). An object just *is* that which is represented when you unite the manifold of intuition so as to yield something thinkable: something you can preface with ‘I think’. Exactly parallel, says Lear, is Wittgenstein’s response to the question how it is that the subjective and the objective points of view are in harmony. *Meaning* seems to be at once a subjective and an objective phenomenon: but for me to mean something by something—to be able to prefix it with an ‘I understand’—it must be part of a—public—*language*. And to share in a language is to share a “mindedness”: the ‘I understand’ can be replaced by a ‘We are so minded’. Again, however, the ‘we’ disappears: for there is no possibility of being minded otherwise than as we are.

In short, on Lear’s reading, the *Investigations* contains materials for a “transcendental deduction” uncannily parallel to Kant’s own. —Now, Lear does not make especially clear what exactly it is a deduction *of*—what corresponds in Wittgenstein’s investigation to Kant’s categories—but he speaks of “establish[ing] the objective validity of our representations” (1984: 233), and it would presumably not do violence to Lear’s account to say, a little more generally, that the deduction is of the objective validity of our “mindedness”. If Wittgenstein’s “grammatical propositions” can

be considered something like the core of his reflective exploration of our mindedness (taking care, of course, not to fantasise this into the kind of “super-order” discussed at *Investigations* §97), then it looks as though Lear is telling us that Wittgenstein is not merely engaged in transcendental philosophy, he has carried it out to something like completion. His transcendental argument establishes the objective validity of our grammatical knowledge: there is nothing for Rödl to bemoan the absence of.

5. Wittgenstein as no idealist

a) Stroud *versus* Lear

Lear’s reading of the later work of Wittgenstein adds a fascinating determinacy to Williams’s earlier, more guarded attribution of a gesture “in the direction of a transcendental idealism” to that work. However, a reader of Lear’s article familiar with the *Investigations* is likely to find amazing the suggestion that such heavy philosophising is to be found there (as that the subjective experience of understanding stands in need of *legitimation*, which it then receives from the insight that the ‘I understand’ which must be able to accompany all my representations is empty until those representations are united into a language, etc.). And I think this sense of amazement is well-founded. Thus Barry Stroud’s symposium contribution, after questioning the details of the “transcendental deduction” Lear claims to find in Wittgenstein, ends with an insistence that an ascription of idealism to Wittgenstein must be wrong. I think some of Stroud’s particular complaints are less than compelling, but I find his conclusion hard to resist. Let’s work through some of the details before considering Stroud’s conclusion.

Stroud first questions whether the ‘I understand’, and likewise the ‘We are so minded’, must by Wittgenstein’s lights be able to accompany all my experiences of understanding, just as the ‘I think’ must, as Kant’s deduction has it, be able to accompany all my representations. Stroud

cites the case of the slab language of §§2ff. of the *Investigations* as a counterexample: for Wittgenstein there—as early as §2, when the language only contains four moves—bids us to “[c]onceive this as a complete primitive language.” The builders cannot preface any of their linguistic acts with ‘I understand:’, or ‘We are so minded.’: for they have no such expressions in their language (not even as it is enriched in the following sections). But then, suggests Stroud, Lear’s transcendental deduction cannot get started.

The slab language is certainly puzzling, and not only because it presents a problem for Lear’s reading. But let’s start with that. A first response to Stroud’s worry might run along these lines: “Surely what’s important isn’t that a *certain form of words* be available to preface any given act of understanding; what matters is *that I understand* it. And the builders surely understand each other, and themselves: for *ex hypothesi*, B acts on A’s commands.” But perhaps it isn’t quite enough just that they understand in this way⁵: for Lear seems to insist, in his explication of the thought that the ‘I understand’ must be capable of accompanying a meaningful linguistic act of mine, that what this signifies is that “I must be able to *take conscious possession of it* for it to be an act of mine” (1984: 228; emphasis added).⁶ Can the builders “take conscious possession” of their linguistic acts? Perhaps the poverty of their language shows, after all, that they cannot, and Stroud’s point is vindicated.⁷

⁵ And we shall question below whether this really does constitute understanding: cf. the sentence below from which note 8 hangs, and the note itself.

⁶ In the (allegedly parallel) Kantian case, it is manifestly important that I am in fact self-conscious: that I do “generat[e] the representation ‘*I think*’ (a representation which must be capable of accompanying all of my representations, and which in all consciousness is one and the same” (B132), even if it is not so important—though Kant doesn’t flag this unimportance—that I use the words ‘I think’ (or ‘*Ich denke*’!) to stand for this representation.

⁷ Goldfarb (1983), crediting Stanley Cavell, suggests that the case of the slab language can be taken in two ways: as the “entire linguistic behavior” of the builders, or as a practice that has a home in a linguistically much richer way of life such as ours. In what follows I presume that we are to take it in the first of these ways. The bulk of what Goldfarb himself takes the case of the slab language to be doing in the *Investigations* is derived from this way of understanding it. But I think nothing essential to my discussion would be threatened by my conceding that Wittgenstein might have in mind that we also consider hearing the example in the second way.

But now we should find ourselves becoming puzzled as to whether we can make sense of this idea of a *language* whose speakers lack the ability to recognise themselves as using expressions with understanding. How seriously, or anyway literally, are we to take Wittgenstein's enjoinder to "conceive [their practice] as a complete primitive language"? Wittgenstein introduces the slab language as an illustration of "the idea of a language more primitive than ours", an idea which he has in turn introduced as something like the practical correlate of the "primitive idea of the way language functions" which is the home of a certain "philosophical concept of meaning" (§2)—namely, of the conception of meaning which has its roots in the picture of language painted in the words of Augustine quoted at the beginning of the *Investigations*. But after all, we are working our way up to the realisation of the *inadequacy* of this conception of meaning and of the "primitive idea of the way language functions" in which it has its place. (One of the milestones on this particular road is §32.) Can it be that Wittgenstein is inviting us to *try* to conceive of the builders' practice as a complete language, so as to see more clearly how it is similar to and different from our own practices (cf. §130)—leaving it open that we may in the end find that we cannot make sense of the idea of a "form of life" to which the language of §2 (or even that of §8) stands in the same relation that our whole language does to ours? (This is, indeed, the sort of structure that Williams and Lear claim to find in much of Wittgenstein's practice in his later work.) If so, perhaps we needn't take too seriously the idea that §2 illustrates a genuinely possible "complete language"—and Stroud's point is once again cast into doubt.

Now, the connection with the idea of a form of life to which I just alluded is made at §19, just after Wittgenstein has urged us, precisely, not to balk at the thought that the languages of §2 and §8 might be complete, on the grounds that it is not clear when we can say of *any* language—even our own—that it is complete. This would seem to pose a problem for the suggestion I just made.

But it might be that what Wittgenstein is especially concerned to discourage in §18 is, as I might put it, a narrowly *surface-grammatical* conception of completeness: thus, it is the fact that the languages of §2 and §8 “consist only of orders” that inspires Wittgenstein to see that we may need reassurance. (Thus, also, in the vicinity we are told that “We could imagine a language in which *all* statements had the form and tone of rhetorical questions; or every command the form of the question ‘Would you like to...?’” (§21).) Perhaps, that is, the emphasis in §18’s reassurance is not on what could or could not make a “complete form of life”, so much as just on whether an apparent poverty of surface grammar should be taken to imply incompleteness. Wittgenstein tells us: “It is easy to imagine a language consisting only of orders and reports in battle.... And to imagine a language means to imagine a form of life” (§19). Yes: it is easy to close one’s eyes and imagine a battle scene, and think about how language is used there, *and not to imagine anything else* in connection with the people involved. The use of language in battle is perhaps distinctive enough that, for certain purposes, we can treat it as an autonomous practice. But must we take Wittgenstein to be committing himself to the genuine possibility of a people who, from birth to death, use language only in the course of doing battle?

In sum: Though it might indeed be rash to attribute to Wittgenstein such a weighty thesis as, say, that “true language use must be accompanied by self-conscious appreciation of one’s understanding of that use”, I think it is also rash to take Wittgenstein’s suggestion to “[c]onceive [the practice of §2] as a complete primitive language” as implying an almost equally weighty thesis to the effect that the idea of a people whose only putatively linguistic practice consisted of what is described at §2 is a fully intelligible one, and that such people could genuinely be understood to be language users. Stroud’s reliance on this case to undercut Lear’s suggestion that, by Wittgenstein’s lights, we must be able to accompany our “representations” with ‘I

understand’ and with ‘We are so minded’ thus strikes me as weak. At any rate, what Lear is interested in is “act[s] of speaking or using a language *with understanding*” (227; emphasis added); on balance it doesn’t seem unreasonable to say, after all, that, whatever the builders are doing (communicating, perhaps), they’re not doing it *with understanding*.⁸

Stroud also has doubts about the last step in Lear’s Wittgensteinian “transcendental deduction”: about the claim that ours is the only mindedness whose possibility we can grasp. Stroud quotes from the *Investigations* to show that Wittgenstein does not hold this:

I am not saying: if such-and-such facts of nature were different people would have different concepts (in the sense of a hypothesis). But: if anyone believes that certain concepts are absolutely the correct ones, and that having different ones would mean not realizing something that we realize—then let him imagine certain very general facts of nature to be different from what we are used to, and the formation of concepts different from the usual ones will become intelligible to him. (*Investigations*, p. 230)

Stroud emphasises that he is not implying, and indeed does not claim, that Wittgenstein believes we have genuine *access* to these other “mindednesses”.⁹ But the very idea of an alternative mindedness doesn’t disappear:

⁸ I may appear to risk sounding as though I am disregarding Wittgenstein’s plea in §154 to “[t]ry not to think of understanding as a ‘mental process’ at all.” (Or anyway, more generally, to risk mystifying “understanding” in the face of Wittgenstein’s attempts to demystify it.) If the builders’ language “is meant to serve for communication” (§2)—and after all *does* so serve (or anyway is so stipulated)—what should prevent us from granting them *understanding*? But I hope the point I am making is in fact compatible with this. Though dogs can be trained to understand commands, no one takes Wittgenstein’s considerations (much less Kant’s transcendental deduction!) to be vitiated if they don’t apply fully to what dogs do. There are differences between what goes on when a dog understands something, and what goes on when *we* understand something—differences which can be specified without appeal to “mental processes”. Thus: we can get confused, and then see our way out of the muddle; we can find something ambiguous, and then see which way to take it; we can ask an interlocutor to explain herself; and so on. (And of course we often understand things straight off, without needing to resort to such helps.) The activity of the builders of §2 resembles that of dumb animals more closely than it does this sort of phenomenon. (Is it the *building*, the structured practice in which their communicative activity is lodged, that distinguishes them from dumb animals? —But animals build as well, even “coöperatively”. If you describe the case in enough detail, in such a way as to reveal an essential difference between what the builders do and what dumb animals do, you will surely make it impossible to hold onto the thought that the language of §2 is their only linguistic activity.) (On this point cf. Rhees (1959-60) on the way in which language games can be parts of *a language*, in apparent disanalogy with games *tout court*.)

⁹ I borrow the wording from the title of Chapter 7 (“Alternative grammars? The problem of access”) of Forster (2004), where he explores this issue at length (though I don’t suppose he is the first to use this phrase). I will return to Forster’s work below.

This [quotation from the *Investigations*] seems to me to acknowledge the intelligibility of there being forms of thinking different from ours. Even if we founder when we try to understand in some detail what it would be like to think in one or another of those ways, so that we do not find fully intelligible any particular way of thinking different from ours, Wittgenstein does seem to be suggesting that we can nevertheless be brought to see the contingency of our thinking in the ways we do, or the contingency of anyone's being 'minded' as we are rather than in some other way. It goes too far, then, to say that for Wittgenstein we cannot even make sense of the possibility of there being other ways of being 'minded'—that 'the concept of being minded in any way at all is the concept of being minded as we are' (1984: 255)¹⁰

But what would be wrong with a response, on Lear's behalf, such as this?: In grasping the fact of our mindedness, we are indeed inclined to think of it as standing in contrast to other "mindednesses". But as we work through cases and find them all unintelligible, we find that the contrast collapses: the idea of an alternative mindedness, as we might put it, "shrinks to an extensionless point." So long as the idea remains purely negative—the idea of a mindedness *unlike ours*—and as it becomes apparent that we *cannot* give it any positive sense, we see that it poses no threat to the sort of argument Lear is trying to construct. —And note the parallel with Kant: though he frequently refers to modes of sensible intuition different from ours, as well as to the idea of an intuitive intellect, in order to render vivid his claims about the nature of our sensibility and understanding, he nevertheless emphasises:

[The act of synthesis of the manifold] is indeed the first principle of the human understanding, and is so indispensable to it that we cannot form the least conception of any other possible understanding, either of such as is itself intuitive or of any that may possess an underlying mode of sensible intuition which is different in kind from that in space and time. (B139)

(I do not pretend that this is unmysterious, but only observe the parallel with the line I am suggesting is open to Lear.)¹¹

¹⁰ Stroud argues at greater length for the same point in his (1965).

¹¹ Lear himself finds a *disanalogy* here: "Diagnosing our consciousness as a discursive intelligence whose sensible intuitions are spatio-temporal, [Kant] is able to contrast us both with discursive intelligences with alternative forms of sensible intuition and with a non-discursive intelligence, an intellectual intuition. Wittgenstein, however, is able to awaken us to the possibility that our form of life is partially constituted by our being so minded without making contrasts with 'other perspectives'" (1984: 232). But I find this puzzling: for by Williams's and Lear's own lights, Wittgenstein gets us started on the reflective exploration of our mindedness by describing (putative, to-be-discarded) *alternatives*; and by Kant's own lights (as we see in the B139 passage, which, though not quoted by Lear, is among the many he lists as references in the footnote attaching to the text I have just quoted) the "alternatives" *he* uses to explicate his ideas are as unintelligible to us as Lear claims Wittgenstein's are. (But see note 17 below for more on the disanalogy.)

Stroud's specific reasons for rejecting Lear's claim to find the materials for a "transcendental deduction" in Wittgenstein's later work are, I thus suggest, less than persuasive. But I nevertheless find irresistible the note Stroud sounds in the last pages of his contribution. Stroud points out that, for Kant, someone looking for a deduction of the objective validity of the categories could not rest content with the result that any thinker must work with them: there appears to be a gap between showing that and showing that the categories are *true to how things are*. And Stroud suggests—and of course this is Kant's claim—that only (transcendental) *idealism* can satisfy the demand to demonstrate the latter, once the demand is granted force. Again, Kant's claim is that we *constitute* objects, we constitute experience, in accordance with the categories. Stroud argues that one who takes Wittgenstein's work to provide a response to this same demand—a response that grants the demand its force and attempts to satisfy it—is bound to find in that work a version of idealism (as indeed Lear does, as we saw (1982: 392)). But Stroud adds, "[t]hat seems to me reason enough to seek some other account of Wittgenstein's philosophy" (1984: 258): for however subtly it is developed, idealism constitutes a conflation of the subjective and the objective in violation of undeniable commonplaces (cf. 1984: 243-244). —And Stroud gives a quick argument for the hermeneutic conclusion: by Lear's own lights, Wittgenstein eschews the kind of detached perspective on our thinking that Kant's idea of a "noumenal world" appeals to: but (*pace* Lear) idealism requires just such a perspective. On Stroud's account, Lear is right that Wittgenstein eschews such a perspective; and Kant is right that the only way to satisfy the demand for a deduction of objective validity is idealism—but put together, these two facts requires us to understand Wittgenstein not as trying to satisfy that demand, but as showing us how we can refuse to grant it force.

b) Idealism in Wittgenstein's early work

Williams motivated his (guarded) attribution of idealism to the *Investigations* by bringing out some of the continuities between that work and the earlier *Tractatus*, and arguing that whatever idealism attached to the elements of continuity as they figured in the *Tractatus* can still be construed to attach, in modified form, to those elements as they figure in the *Investigations*. (Again, the attribution of idealism was in both cases guarded: the “need... to try to point, hopelessly, in a solipsistic direction” of the *Tractatus* takes the form, in the *Investigations*, of a “point[ing] in the direction of a transcendental idealism” (Williams 1974: 147, 163).) One way of undermining the ascription, guarded or not, of idealism to the later work would therefore be to undercut the claim (guarded or not) that it is to be found in the earlier. It would take us a little too far out of our way to delve deeply into *Tractatus* interpretation here; let me therefore simply gesture at the existence of a coherent and satisfying reading of the *Tractatus* that finds in it an unequivocal *resistance* to idealism.

Peter Sullivan has argued that part of the achievement of the *Tractatus* is the “demonstrati[on] in practice... that transcendental idealism is untenable.”¹² We come to see, as we work through the *Tractatus*, that:

the notions of world and thought are... intrinsically tied, that the world is not something *other*, so that it would need the kind of positive philosophy aimed at establishing an a priori order to ensure thought's engagement with it. (1996: 204)

But if this is right—if the engagement with transcendental idealism in the *Tractatus* issues in its dissolution rather than its (silent) advocacy—then the story of Wittgenstein's development that Williams tries to tell gets it wrong from the beginning. Williams writes that a consequence of an

¹² See e.g. Sullivan (2004); the quotation is from p. 43.

idea to be recovered from the *Tractatus* is that, in the senses of ‘world’, ‘language’ and ‘my’ at issue in the context of the discussion of solipsism:

we cannot conceive of it as a matter of empirical investigation... to determine why my world is this way rather than that way, why my language has some features rather than others, etc. (1974: 146)

Williams appears, however, to be implicitly assuming that those questions nevertheless have, in the *Tractatus*, some go: only, not as “a matter of empirical investigation,” but rather, one supposes, as a matter of *transcendental* investigation.¹³ (This grounds Williams’s inclination to find in the *Tractatus* a “need... to try to point, hopelessly, in a solipsistic direction.”) But if Sullivan’s work is on the right track, it looks as though the *Tractatus* is showing us how to stop supposing that there is an *a priori* order of things (cf. §5.634): showing us that, once empirical matters are set aside, there is simply nothing left of the questions Williams mentions.

c) Idealism in Wittgenstein’s later work

i. No justification...

Of course, if Williams has misunderstood the thrust of the *Tractatus*, that does not in itself demonstrate that he is wrong to find a gesture toward idealism in Wittgenstein’s later work; so we ought to direct our attention to the latter, to see if we can satisfy ourselves that we aren’t forced into the understanding of it that Williams and Lear recommend. —Stroud essentially *tells* us that it must be wrong to find idealism there—more on the grounds that idealism is a philosophical dead end than on the strength of a positive exegetical effort¹⁴ (but note the similarity between, on the one hand, the sort of response Stroud suggests we ought to look for in

¹³ I owe this thought, and indeed the connection to Sullivan’s work, to John McDowell.

¹⁴ but we also mentioned above his quick argument that Wittgenstein’s later work is not idealist, grounded on Lear’s own observation that Wittgenstein does not recognise the intelligibility in any sense of a vantage point external to our mindedness

Wittgenstein's later work to the demand for a deduction of objective validity, and on the other, Sullivan's reading of the relation of the *Tractatus* to idealism); but we need to think this through.

Perhaps a way to start is by recording the emphasis Wittgenstein places on the unavailability of *justification* for our mindedness. (I will continue to use Lear's term, for convenience.) Indeed, Williams, and Lear in turn, themselves allude to this feature of Wittgenstein's thought: thus Williams finds in Wittgenstein's refusal to explain our mindedness, or compare it evaluatively with others, materials for the claim that Wittgenstein does not really mean his discussions of alternative practices to be taken to present genuine alternatives, and that his view is thus importantly distinct from relativism. But, though Williams recognises that empirical explanation and justification are not at issue, it looks as though he is thinking that there is *some* sort of—nonempirical—explanation or justification still to be intelligibly hoped for. This is parallel to the point about Williams's reading of the *Tractatus*.¹⁵ Williams calls “the fact that our language is such and such, and... that the world we live in is as it is” “transcendental facts”, and remarks that they thus “have no empirical explanation”; but he goes on to suggest that the rôle played by our reflective exploration of our world view stands to these “transcendental facts” as empirical explanation would to corresponding empirical facts (about particular languages, say), and indeed to claim that it is precisely this that justifies calling Wittgenstein's later views “a kind of idealism” (Williams 1974:152-153).

But some of the features that Williams himself emphasises, in his excursus on relativism, seem to tell against this suggestion that, by Wittgenstein's lights, any (even inarticulable and transcendental) explanation is available. Williams highlights Wittgenstein's insistence that any putative explanation of our world view, or evaluation of it in comparison with an (alleged)

alternative, would itself have to be made from within our world view. He collects several passages from Wittgenstein's writings; let me reproduce part of Williams's result:

There are many remarks, again, which claim such things as that reasons can be given only within a game, and come to an end at the limits of the game (*Philosophische Grammatik*, p. 55), that our mode of representation is a language-game (*Philosophical Investigations*, p. 50), that 'grammar' cannot be justified (*Philosophische Bemerkungen*, p. 7), and that the language-game is not reasonable or unreasonable, but is there, like our life (*On Certainty*, 559). (1974: 156)

No doubt Williams supposes that the reflective exploration of our world view that Wittgenstein's investigation of our practices constitutes and encourages us to carry on—or, rather, the reflective clarity that such exploration results in—is in some way exempt from the limits he canvasses: perhaps the difference is thought to lie in the peculiarly *reflective*, consciousness-raising quality of this clarity, with the implication that it transcends any particular aspect of our practices (maybe by grasping them all together in a *Gestalt*). But this is hard to reconcile with what Williams himself told us earlier about the result of this exploration: namely, that “[w]hat one would become conscious of, in so reflecting, is something like: *how we go on*” (1974: 153; Williams's emphasis). It is very hard to see how becoming conscious of how we go on can yield anything that can play an (even transcendently) explanatory rôle *vis-à-vis*—what was after all the (transcendental) explanandum—“the fact that our language is such and such, and... that the world we live in is as it is.” For the fact that our language is such and such just *is* a matter of how we go on. These are too close together for the one to constitute an explanation of the other. (Whether we ought to call the fact that the world is as it is a matter of how we go on—indeed, whether it is properly conceived of as an explanandum at all in this connection—depends on how we resolve the question of idealism, which is just what is at issue.)

¹⁵ This observation, and more generally this paragraph and the next two, owe much to some remarks of McDowell's.

And some of Wittgenstein's insistence that we not press the search for explanation too far seems general enough to suggest that we ought to abandon the thought that even transcendental explanation is available. Rather than quoting a barrage of well-known passages about how "explanations come to an end somewhere" or how "my spade is turned" (§§1, 217)—remarks which Wittgenstein makes, by the way, in the context of discussions of cases of *how we go on*—let me quote at length one passage from the *Investigations* which I think illustrates nicely the kind of suspicion Wittgenstein would have of the idea of the availability of an explanation or justification (however ineffable or nonempirical) of our mindedness. (The context is a discussion of the idea that, in explaining the meaning of particular words I use, I have to use words whose meanings might themselves be enquired into in turn; but I think the remark can be understood much more generally.¹⁶)

“But then how does an explanation help me to understand, if after all it is not the final one? In that case the explanation is never completed; so I still don't understand what he means, and never shall!”—As though an explanation as it were hung in the air unless supported by another one. Whereas an explanation may indeed rest on another one that has been given, but none stands in need of another—unless *we* require it to prevent a misunderstanding. One might say: an explanation serves to remove or to avert a misunderstanding—one, that is, that would occur but for the explanation; not every one that I can imagine.

It may easily look as if every doubt merely *revealed* an existing gap in the foundations; so that secure understanding is possible if we first doubt everything that *can* be doubted, and then remove all these doubts. (§87)

Surely the philosophical motivation for the adoption of an idealism lies in its promise to explain or justify the adequacy of our thinking, of our claims to knowledge. (Again, this is explicit in Kant.) But if Wittgenstein explicitly denies the availability of such justification—and if claims to find, in the face of this explicit denial, an implicit (perhaps “hopeless”) pointing in the direction of such justification are weak—it is hard to see the grounds for attributing idealism to him.

¹⁶ For passages denying the possibility of justification explicitly in the case of grammatical knowledge, see Forster's references at (2004: 30-31). I shall discuss Forster's interpretation of these passages below.

And it is hard to resist emphasising again the point that Stroud makes about external vantage points. Lear, again, insists on the disanalogy between Kant's willingness to advert to a standpoint outside our forms of thought and Wittgenstein's refusal to allow that we can make sense of such a standpoint; Lear's purportedly Wittgensteinian "transcendental deduction" thus makes no use of such a standpoint.¹⁷ But I find it hard to see how Lear can get around Stroud's objection that, whatever reflective clarity that sort of moving around inside one's world view can yield, it cannot get a grip on the question whether that world view, considered as world view, gets things right about the world, considered independently of any world view. If Lear is right, that is, to insist on Wittgenstein's rejection of the very idea of an "external vantage point", it's hard to see why such rejection doesn't just come to a refusal to indulge the inclination to ask the sort of question to which a transcendental deduction of objective validity purports to provide an answer.

- ii. ... over against alternatives?: Forster on Wittgenstein on the arbitrariness of grammar

But perhaps we needn't explicitly take up an "external vantage point" to get the worry about the adequacy of our mindedness going: perhaps the mere idea of *alternative* mindednesses suffices to get us worried—and perhaps this idea is available to us from within our mindedness. Thus far, I have mostly left unquestioned Williams's and Lear's claim that Wittgenstein does not really mean the "alternative practices" he describes to be taken seriously as genuine alternatives ("to

¹⁷ Back in note 11 I suggested that it was *anomalous* for Lear to insist on the disanalogy. But the dialectical context was an initial defense of Lear's Wittgensteinian "transcendental deduction" against objections. If one is concerned to establish the objective validity of our conceptual apparatus, one had better show that genuinely alternative apparatus are nothing (or anyway nothing to us): and both Kant and Lear's Wittgenstein have this, in one form or another. But my point here is that, for the attribution of idealism to stick, there had better be another level at which we can hold our own cognitive activity at arm's length to observe how it produces the world of experience; and here, Lear's own observation of the disanalogy between Kant and Wittgenstein becomes telling.

us” rather than merely “for us”, in Williams’s phrase). I briefly discussed Stroud’s claim that, for Wittgenstein, at least the very idea of an alternative mindedness is left intact, even if the content of a particular alternative is in principle unavailable to us; but only to suggest that such a thin idea might not be enough to generate a problem for the sort of argument Lear wants to run. But Michael Forster has recently published work in which he argues that Wittgenstein really means to have shown that there are genuine alternatives to our practices, to our very mindedness: we can and indeed do know that ours is not the only way to think.¹⁸ Forster accepts Stroud’s claim that such alternative world pictures need not be *accessible* to us; but he takes our (alleged) ability to recognise their genuine possibility to be sufficient not only (as Stroud holds) to undercut a Lear-style “transcendental deduction” of the objective validity of our own way of thinking, but to establish firmly a genuine conceptual pluralism incompatible with the thought that there is no meaningful question to be asked about the adequacy of our thinking to reality.¹⁹

I think Forster’s emphasis on our inability, by Wittgenstein’s lights, to *justify* our ways of thinking is salutary: for, though (as we saw) Lear too recognises this, he takes it to be compatible with an (at least implicit) transcendental argument for the legitimacy of our ways of understanding; and this seems like an awfully unstable position. But Forster, before addressing the question of justification, argues that Wittgenstein is committed to what Forster calls “the diversity thesis”: argues that “Wittgenstein believes that for all grammatical principles...

¹⁸ (Forster 2004). His attack on Davidson’s “On the very idea of a conceptual scheme” (Forster 1998) is of a piece with this work.

¹⁹ I mean, by putting things in this way, to leave it open for Forster to claim that the (by his lights) meaningful question about adequacy, though askable, is not satisfyingly answerable. (One result of denying transcendental idealism can of course be skepticism.) But—though he thinks Wittgenstein is more or less on the right track in his doctrine of unjustifiability—Forster suggests that some considerations Wittgenstein laid down (according to Forster’s reading) as necessary criteria of the very meaningfulness of putative grammatical principles are better construed as matters of degree, deployable in limited ways in adjudicating between competing principles. But this of course simply reinforces the point that, by Forster’s lights, Wittgenstein’s “conceptual pluralism” makes questions about adequacy intelligible.

govern[ing] our ‘true-false games,’ alternative but in some degree similar grammatical principles... either have actually been used or are at least possible and conceivable” (2004: 21). And this colours his understanding of the no-justification idea: for Forster understands the idea of justification in terms of justification *over against alternatives*. He tacks this qualification on just about every time he speaks of justification (see *e.g.* 2004: 30ff.), and reads it into Wittgenstein’s own discussions of arbitrariness and justification even where Wittgenstein has said nothing corresponding. Indeed, though Forster presents his book as an exploration of the sense in which, for Wittgenstein, grammar is arbitrary, its centrepiece is clearly the “diversity thesis”; and Forster spends ten pages adumbrating this thesis—in explicit answer to the question what Wittgenstein means by grammar’s arbitrariness—before he reveals that there are points in Wittgenstein’s texts where Wittgenstein himself explicitly raises the question what he means by calling grammar arbitrary, and answers that he means that it cannot be justified (often not, *pace* Forster, “over against alternatives” but *simpliciter*).

The exegetical practice strikes me as disturbing; but presumably Forster thinks it harmless, since he thinks Wittgenstein holds the “diversity thesis” in any case. I want to leave open for the moment the question whether this is right, and hence to avoid building the presupposition that it is into the discussion of the issue of justification. But someone might ask me what on earth justification can *be* besides justification “over against alternatives”. To this, a Lear might respond that, sometimes, justification can consist precisely in the demonstration that there *are* no alternatives (rather than that, say, ours is preferable to the others): the demonstration that the ‘we’ disappears is the final step in the Wittgensteinian transcendental deduction. (Of course, in claiming this Lear has to be careful about the sense of ‘justification’ at issue, since after all this comes to finding in Wittgenstein’s work a kind of justification of something which, Wittgenstein

tells us explicitly, has no justification.) And Stroud, who resists Lear's attribution of such an argument to Wittgenstein, can agree at least with the thought that justification needn't be "over against (substantive) alternatives". Indeed, as we saw, that constitutes the basis of his claim that Lear's "transcendental deduction" fails: for even having shown that any thinker must think as we do, it still looks askable whether how we think corresponds to how things are; questions of justification, that is, still arise. (Stroud's example is a Humean treatment of causation: we could imagine someone insisting that any thinker, just *qua* thinker, must acquire the "determination of the mind" which Hume describes—the custom of associating ideas of objects that occur in constant conjunction—while denying, to the dismay of a Kant, that any (further) "objective necessity" corresponds to this association.) No doubt the (at least notional) availability of alternatives renders the question of justification more vivid; but it looks as though it is, strictly, independent of such availability.

So it seems that there is room for someone to hold that there are no genuine alternatives to our grammar (to our mindedness), but that neither this absence of alternatives nor anything else *justifies* our grammar. But if Forster is right that Wittgenstein espouses the "diversity thesis", then Wittgenstein, at least, does not hold this. Is Forster right?

In order to establish that Wittgenstein is committed to the "diversity thesis", Forster cites a number of passages in which Wittgenstein apparently commits himself to it explicitly. Now, I cannot address all the passages Forster quotes (and the same goes for the less textually rooted arguments Forster gives, some of which I shall consider briefly below); but I think it is fair to say generally that Forster's treatment of such passages is too quick, in two senses. First of all, I think Forster is too easy on himself when it comes to identifying cases of—in Wittgenstein's

words—“*essentially* different concepts” (Zettel §388; Wittgenstein’s emphasis). For instance²⁰, at several points Forster discusses the case (which Wittgenstein himself discusses at several points) of a different kind of negation from ours: one which does not support the rule of double negation elimination. Now, wherever one stands on the question of the genuine possibility and intelligibility of “essentially different” conceptual systems, surely no one should deny that attributing to someone such a system—say, one with a different underlying logic—will be a sensitive matter, to say the least. But Forster, revealingly, has this to say, in an aside, about the question of negation:

[D]ialects of English sometimes seem to use a variant of negation which is unlike the classical logician’s “~,” for instance in failing to respect the law of double-negation elimination (rather as in Wittgenstein’s own example)—hence in dialect “He ain’t no fool” means that he is not a fool, not that he is one. (2004: 227n21)

The idea that this is a genuine instance of “failing to respect the law of double-negation elimination”, and thus of a concept of negation distinct from the classical logician’s, seems too preposterous to bother refuting. But just to make my thought explicit, note that Forster’s suggestion that the “dialect” sentence involves double negation would seem to imply that it is the negation of “He is no fool” (or perhaps of “He ain’t a fool”). Would one who utters Forster’s sentence take himself to be negating either of these? —Applying these principles of analysis, I suppose Forster would find that standard varieties of French lack negation almost altogether, except in the context of double negation—where (surprise!) the result of double negation is not equivalent to the contained, unnegated sentence.

I do not take myself to have undercut everything Forster has to say about double negation. I mean only to have illustrated my worry that Forster finds it easier than he ought to discover cases

²⁰ This may be the least favourable instance to Forster of his entire monograph; perhaps it is uncharitable of me to seize on it. But it will at least make as clear as possible the worry I have in mind.

of (essential) conceptual distinctness. To give a somewhat more determinate shape to my worry, let me allude briefly to Forster's allied attack on Davidson's claim that we cannot make sense of the idea of radically different conceptual schemes (Forster 1998). There Forster raises, no doubt properly, the question just what "radical difference" should be taken to come to in this context; but in his brief discussion of this question (1998: 135-136 and 170n14) he makes use of the ideas of *quantity* and *quality* of differences between sets of concepts without explanation, as though these ideas were straightforwardly intelligible. I cannot see that they are—surely we are not, for instance, meant to count single *words* and take the result to yield a quantification of conceptual resources!—and indeed, I take it that it is thinking about what it would take to make such ideas precise that leads one down paths like Davidson's. I am inclined to think that Forster's refusal to go down such a path rests in part on his failure to take seriously this sort of challenge. But I shall have to leave detailed discussion of Forster's piece on Davidson for another occasion.

I suggested above that there were two senses in which Forster's treatment of passages in Wittgenstein apparently supporting the "diversity thesis" was too quick. I have discussed the first sense: he is too quick to take any old (putative) difference in conceptual apparatus to be an instance of "*essentially* different concepts". The second sense is simply that I think many of the passages in Wittgenstein's work that Forster cites are less unambiguous than he suggests: it is not as clear as he claims, for instance, that they are straightforwardly opposed to the Williams-Lear reading according to which putative alternatives are not meant as genuine alternatives so much as as foils for facilitating a better understanding of our own practices. Here is an illustration of what I have in mind, in which Forster cites a passage from *Zettel* as unequivocal support of the diversity thesis:

[Wittgenstein] argues that someone might be able to dispense with our prohibition against speaking of a reddish green and instead recognize something of the kind: “These people are acquainted with reddish green.—‘But there *is* no such thing!’—What an extraordinary sentence.—(How do you know?)” (Forster 2004: 23, citing *Zettel* §362)

Forster does not register the delicacy with which Wittgenstein resists the objection, “But there *is* no such thing!”—indeed, he takes Wittgenstein to be *arguing* that “someone might be able to... recognize [reddish green]”! (We can bring out Wittgenstein’s delicacy a bit by observing that, a few pages earlier, he has said that “‘There is no such thing as a reddish green’ is akin to the sentences that we use as axioms in mathematics” (*Zettel* §346). It does not seem unnatural to remark of, say, ‘Things which are equal to the same thing are also equal to one another’, “What an extraordinary sentence.—(How do you know?)”—without thereby implying that one *doubts* that things which are equal to the same thing are equal to one another, much less that one means by this remark to *argue* that they *aren’t*.) Surely what is going on in this area is that Wittgenstein is resisting a certain kind of explanation, that we can feel inclined to give, of the impossibility we find here (cf. *Zettel* §331)—not that he is resisting the very idea that there is a kind of impossibility.

Forster does the same thing, in the previous chapter, with a closely related passage from the same discussion in *Zettel*:

Wittgenstein’s [position] entails that many fundamental, necessary features of our experience which we are prephilosophically inclined to ascribe to a world independent of our minds... turn out instead to have their source in our minds. (2004: 15)

Forster calls this idealism, and illustrates it with this passage from Wittgenstein’s text:

We have a colour system as we have a number system.
Do the systems reside in *our* nature or in the nature of things? How are we to put it?—*Not* in the nature of numbers or colours. (*Zettel*, §357)

Forster, again, takes it that the quotation speaks for itself, unambiguously: he takes it, I suppose, that Wittgenstein, having rejected one limb of the disjunction he offered, must simply be

accepting the other. This is helped along by Forster's having misquoted the passage: where Wittgenstein says (as I have quoted here) "*Not* in the nature of numbers or colours", Forster has him conclude instead "*Not* in the nature of things", which really does look like one of the disjuncts. But by switching from 'things' to 'numbers or colours', it is conceivable that Wittgenstein might be deliberately leaving open the possibility that "in the nature of things" wouldn't be a wrong answer, even if "in the nature of *colours*" (say) is bound to be misleading. But even setting the problem of Forster's unfortunate misquotation aside, and supposing that Wittgenstein does mean to be rejecting one of the initial disjuncts, it is surely clear that he is not comfortable straightforwardly accepting the other. Surely this indicates a mistrust of the *question*: not a mere reluctance (to be overcome) to give it the "prephilosophically" more surprising answer.²¹ (Note, incidentally, the affinity of this reading of the *Zettel* passage with Sullivan's reading of the *Tractatus* and Stroud's suggestion for making sense of Wittgenstein's later work, discussed above.)

We have considered at some length Forster's arguments for the claim that Wittgenstein holds the diversity thesis. I cannot pretend to have dispensed with all of them (nor do I have the space to do so). But Forster himself does put a lot of weight on the "textual evidence" he cites at the beginning of his Chapter 2, and I have suggested that it won't bear all that weight. He gives further arguments as well; I don't find them any more convincing. Let me discuss just one more instance: he asserts that, of the three purposes he finds in Wittgenstein's discussions of alternative grammatical practices, two require that the alternatives be genuinely possible. In brief, the three purposes in question are, first, "to discredit a form of Platonism... which holds

²¹ I shall not here pause over Forster's further contentious move, to infer from "in our nature" to "in our minds", and to call the resulting position idealism.

that concepts are *sparse*, as it were”; second, “to depict a possible²² alternative in order thereby to make clearer by specific contrasts or similarities some feature of our own grammar”; and finally (a purpose which, Forster concedes, is amenable to Williams and Lear’s reading), in “a small class of examples..., to unmask seemingly coherent philosophical conceptions as in fact implicitly incoherent” (2004: 25; italics in original). But even granting this list of Wittgenstein’s purposes, I simply don’t see why Forster thinks the first two require that the described cases be genuinely possible, rather than (say) the kind of ultimately empty foil that Williams and Lear find in them. He gives no argument for this claim; I suppose he takes it to be obvious. But (taking the second purpose first) our reading in §5. a) above of Wittgenstein’s discussion of the slab language seems to provide a perfectly intelligible case of describing a putative “alternative” “in order thereby to make clearer by specific contrasts or similarities some feature of our own grammar”, without any implication that the “alternative” in question is a genuinely possible language. (Indeed, we argued above that it is not.)²³

As for the first purpose, Forster does not really explain what he means by the thesis of concepts’ “sparseness” (and it is perhaps especially unclear given that he is at pains to distinguish the thesis from the (also “Platonic”) idea “that meanings are eternal objects [and] ontologically independent” (2004: 204n4)). He elaborates on the thesis only as far as adding: “that there is just one concept of Virtue, one concept of Justice, and so on” (2004: 204n4). But we can learn a

²² Of course, by inserting ‘possible’ into his statement of the purpose, Forster has settled it that the cases Wittgenstein describes with this purpose in mind are “possible”!—but this is clearly mere sleight of hand on Forster’s part. What we ought to ask, plainly, is whether the goal of “mak[ing] clearer by specific contrasts or similarities [with a described alternative] some feature of our own grammar” requires that the alternative be genuinely possible; so this is how I shall take this second purpose in my discussion below.

²³ Does this just show that the slab language is not an *alternative* to ours, and hence that it doesn’t count as a case of Forster’s second purpose? —But unless this worry is just the sleight of hand warned against in note 22 above, this can only be to say that Wittgenstein’s goal in describing the slab language is not to illuminate anything about our own language in contrast—but now compare *Investigations* §130, where Wittgenstein states in the clearest possible terms that that *is* precisely his goal in discussing the “clear and simple language-games” such as the one in §2.

little more about what Forster has in mind when he gives, as an example of the view that concepts are “sparse”, “[Frege’s] notorious judgment that even ‘and’ and ‘but’ are identical in sense” (2004: 205n4).²⁴ Even this is not fully clear; it is not clear why denying Frege’s “notorious judgment” could not mean simply finding different (still “sparse”) concepts associated with the two words.²⁵ But in any case, it is telling, I think, that ‘and’ and ‘but’ are, after all, both “*our*” concepts (or, if you prefer to think of them—with Frege—as takes on the same concept, both “*our*” takes). Whatever precisely it comes to to find in Wittgenstein a rejection of the view that concepts are “sparse”, surely the discussion of family resemblance in the *Investigations* is a central expression of this rejection²⁶; and what it is a rejection of is a certain picture of the working of *our* concepts. That is, the thought is not that, while *our* concepts form a sharp, clear order as of the purest crystal (cf. §97 and *passim*), the comparison with *others’* concepts reveals that concepts “as a whole”—“theirs” and “ours” taken together—are not sparse: rather, it is that it is a fantasciation to suppose that our own concepts form that sort of order. Thus, again, I do not see why Forster believes that Wittgenstein’s motivation of this thought depends on the genuine possibility of the alternatives he describes.

Even though it cannot be said that I have addressed every consideration raised in Forster’s monograph, I hope I have succeeded in making it plausible that Forster is not successful in establishing Wittgenstein’s commitment to the “diversity thesis”.

²⁴ This is a little awkward as Frege exegesis, since for Frege what settles it that we express the same concept with ‘and’ and ‘but’ is that they are identical in *reference* (that is, they *refer* to the same concept), not in sense (though of course it is also true that Frege holds them to be identical in sense). But since Wittgenstein does not adopt this Fregean apparatus unchanged, we can perhaps forgive Forster for the awkwardness that arises from trying to discuss the two authors using the same vocabulary.

²⁵ I suppose the thought must be that ‘and’ and ‘but’ both express the concept of *conjunction*. One who holds the “sparseness” thesis must either (like Frege) insist that the two words express the same concept *tout court* (no ifs, ands or buts), or else deny that they express the *same* one concept in *any* sense; whereas to let go of the “sparseness” thesis is to allow that ‘and’ and ‘but’ express different *concepts-of-conjunction*. This is the best I can do at Forster exegesis; but the point I go on to make in the text stands.

What I hope I have made room for is a position (sketched above but threatened by Forster's attribution of the "diversity thesis" to Wittgenstein) something like this: Williams and Lear may well be right that we are not forced to take Wittgenstein to be committing himself, through his discussion of alternative practices, ways of thinking, and so on, to the genuine possibility of thinkers for whom those alternatives are real (much less to their straightforward intelligibility to us)—and so we are not forced to find in him a simple conceptual relativism, and a concomitant crude empirical idealism, as Forster does.²⁷ But at the same time, we can agree with Stroud (and against Lear) that the disappearance of the 'we' does not in itself constitute any kind of *justification* of our ways of thinking: for, precisely because (as Lear recognises) no external vantage point from which to consider the question of the fit between our concepts and the world enters into the considerations leading up to the 'we's disappearance, no answer to that question is either intelligible or required. But such justification is what idealism is after: to suppose that we constitute the fit between our concepts and the world only has an appeal for us if we begin by finding them at a distance from one another.

6. Motivations for finding idealism in Wittgenstein

Perhaps my suggestion that the later Wittgenstein's work is best not thought of as idealist will be a little more satisfying if I say a bit more, in an accommodating sort of spirit, about why it is tempting to find a kind of idealism there. There is no doubt, first of all, that Wittgenstein *looks* idealist sometimes: for instance in those *Zettel* discussed above, where he discourages finding (the basis of) our *systems* of colours and numbers "in the nature of numbers or colours."

Anscombe, in her discussion of such passages as these (1976), distinguishes between holding

²⁶ E.g., §69: "We do not know the boundaries [of the concept 'game'] because none have been drawn.... [W]e can draw a boundary—for a special purpose. Does it take that to make the concept usable? Not at all!"

that “essence is *expressed* by grammar” (*Investigations*, §371, emphasis altered) and holding that essence is *created* by grammar: and suggests that in a great many cases, there is no need to infer the second from the first. But she is surely a bit quick in her insistence on this point. For example, she tends to make the point in terms of counterfactuals, in this sort of way (the context is a discussion of different concepts of pain):

Now what is there in all this to make a difficulty about saying: “Even if there had never been any human language so that there *was* ‘no concept of pain’ at all – still, if there were animals, there would have been pain?” Nothing....
And similarly, if there never had been humans around talking about horses, that is not the slightest reason to say there wouldn’t have been horses. (1976: 114)

But (though this is less clear in the case of the sort of idealism that Forster wants to attribute to Wittgenstein) Williams made explicit—and Lear picked up on this and emphasised it further—that the idealism they find in Wittgenstein’s work is from the beginning not such as to imply the truth of any such counterfactual. (Perhaps it expresses this to say it is transcendental, not empirical, idealism.) As we saw Williams put it before:

... *our* language, in [the] sense in which its being as it is has no empirical explanation, shows us everything as it appears to our interests, our concerns, our activities, though in the only sense in which we could meaningfully say that they determined everything, that statement would be false.... [T]hat provides grounds, I suggest, for calling such a view a kind of *idealism*.... (1974: 153)

In any case Anscombe herself concludes that there *is* an element of idealism to be found in Wittgenstein’s later philosophy as well: for even if statements about horses, for instance, and their colours and so on are not impugned by the considerations touched on in the *Zettel* passages (among others), grammatical propositions themselves are (expressions of) *rules*, and Wittgenstein, says Anscombe, “was a linguistic idealist” about rules.²⁸ She arrives at this result by recommending the following “test question”: “Does this existence, or this truth, depend upon

²⁷ I suppose Forster would resist this characterisation of the idealism he imputes to Wittgenstein. (For instance, he emphasises that it is of substantially the same character as Kant’s.)

human linguistic practice?” (1976: 116). And no doubt Wittgenstein held that the existence of rules depends upon human practice.

Certainly Wittgenstein held that “‘obeying a rule’ is a practice” (*Investigations*, §202)—and, further, this is not to say just that *obeying* a rule is a practice (one, say, to be understood as guided by “the rule itself”, intelligible independently of a grasp of the practice), but that *obeying a rule* is a practice: that to grasp a rule *is* to grasp the practice in which it has its home. But this is meant as a reminder, not as a heavy thesis: we learn rules by learning the practices they guide, and we know “what a rule is” because we know what a rule-governed practice is. (On the commonplace-ness of this idea, compare Anscombe on Hume’s claim that “promises have no force antecedent to human conventions”: “If moralists have found this offensive, this will have been by misunderstanding it” (1976: 119).)

But just this commonplace-ness should tip us off to the fact that there is something odd in finding here an *idealism*. Doing so would suggest that there is a peculiar *puzzle* about the relation between us and rules—a *chasm* between us and them—which we can fill by seeing that, as it’s *we* who *create* the rules, it’s no surprise that we can know them and act in accordance with them. No doubt one can get puzzled in this sort of way when one realises that one cannot license a rule about, say, refusing to call anything reddish green simply by *pointing at red* and *pointing at green*. (Cf. *Zettel*, §331.) But compare Wittgenstein’s treatment of rules, sign-posts and so on in the *Investigations*. If one thinks of a sign-post as itself “normatively inert,”²⁹ one will be puzzled as to how we succeed in settling on one interpretation of it rather than on any of

²⁸ Forster argues that if Anscombe concedes this much, she ought to recognise that such idealism will infect even ordinary empirical statements: for by Wittgenstein’s lights, our grammatical principles are not merely laid over an independently given reality, they “[regulate] which... factual judgments it is appropriate for us to make” (2004: 17).

²⁹ I owe the phrase to John McDowell. It will be seen that this and the next few sentences are meant to follow McDowell’s account of Wittgenstein’s rule-following considerations as he puts it forward in *e.g.* his (1993).

countless others (and as to how we *settle* on an interpretation without needing to interpret *it* in turn). But Wittgenstein's solution isn't to suggest that our experience of sign-posts as normatively potent is, at a transcendental level (where, I suppose, the sign-posts "really are" inert), constituted by our activity. It is simply to refuse to allow that the sign-post is, at any level, normatively inert: it is (ultimately) to refuse the puzzlement. It has a place in our activity *as a sign-post*; and so understood, it is normative "all the way down". "[T]here is a way of grasping a rule which is *not* an *interpretation*" (*Investigations*, §201). Returning to our original case: it's *not* that we start with inert stuff—things with colours, which we can recognise as things with colours independently of any grasp of *rules* for the use of colour concepts—and then ask: "How do we know there's no reddish green? Does *that—red*—somehow *tell* us?" (If we get as far as asking this question, we have to answer No: if we "run up against existence and non-existence", it will be "against *facts*, not concepts": *Zettel* §364. But—the point—to answer 'No' here isn't to answer 'Yes' to the next question:) "If not, do *we* as it were tell *it*?: do we *make* it so?" Surely this is no more satisfactory. Just as "the sign-post does after all leave no room for doubt" (*Investigations*, §85)—it points the way, *and I go*—so my interactions with and judgments about things proceed according to rules. (Asked of *olive green* to "Name two sensations which *this* is between," "one will *not* get 'red and green'" (*Zettel*, §§360-361).) We cannot effect the separation that the puzzlement presupposes. But I cannot see why it is perspicuous to call a view *idealism* unless it purports to provide a bridge across a gap it construes in that sort of way.

7. Is the *Investigations* in need of a transcendental vindication?

We have so far not addressed directly the question I with which I began this chapter: namely, whether Rödl's complaint against Wittgenstein is well placed. We have focused our attention on the more limited goal of determining just what Wittgenstein's understanding *is* of the place of

“vindication” in his own work, against which understanding Rödl aims his complaint. But I think we have amassed the resources required to make a start, at least, at addressing the bigger question.

Consider, first, what it would come to to “adjudicate” between Wittgenstein’s self-conception (as we have now understood it) and Rödl’s claim that further justification of the grammatical remarks is required. There is something puzzling about the very demand even to do this: after all, to *argue* (successfully) in favour of Wittgenstein here would surely constitute a kind of *justification* of the “grammatical remarks”—but it is precisely this whose feasibility Wittgenstein denies! It seems as though, whichever side we set out to defend, Rödl is somehow guaranteed to end up vindicated—but this is surely a kind of sleight of hand. Surely “explanations come to an end somewhere” (*Investigations*, §1); the question can only be where.³⁰ To put the distinction between Wittgenstein’s and Rödl’s conceptions of the status of the *Investigations*’s “grammatical remarks” in slightly different terms, then, it must simply be the difference between comfortably *accepting* them, without feeling as though they call out for further justification (especially on the basis of some one principle), and (on the other hand) finding oneself warranted in demanding further explanation of their grounding. (Presumably part of Wittgenstein’s point in calling them “reminders” (§127) is to flag them as remarks whose truth we instantly recognise, rather than feeling any inclination to doubt.) But how can we go about settling *this* difference in turn?

Well, the considerations I’ve been developing at least suggest that there’s room for a kind of accommodation of Rödl’s project in *Kategorien des Zeitlichen*, which he conceives of as part of “transcendental logic”, that doesn’t concede his main complaint. These considerations suggest that we needn’t be haunted by the spectre of competing, (“essentially”) alien ways of thinking

about, and moving about in, the world—and, of a piece with this, that our “ways of thinking about, and moving about in, the world” are not something from which we can separate ourselves, or the world, far enough to allow ourselves to be troubled by the prospect of their marching out of step with the world. But this isn’t at all to say that those “ways” have no *structure*: as we saw at the beginning of this chapter (and in *Investigations* §92), there is structure there, indeed structure “worthy of recovery” (so long as we understand ‘recovery’ in a sense not so much of revelation as of recollection). But then perhaps the thoughts upon which Rödl himself insists in his work on temporal categories—for instance, the thought that the threefold formal distinction among “is doing”, “was doing” and “did” cannot profitably be reduced to the twofold distinction between “is” and “was”; the thought that that twofold distinction in turn cannot profitably be reduced to tenseless “is”; and so on—constitute a part of just this exploration.

Rödl takes it that if an aspect of a judgment of experience is not part of its *content*, it must be part of its form, and hence—the punch line (for my purposes)—“*in us*”.³¹ Though the distinction between content and form is perhaps innocuous³²—Rödl argues persuasively (in his Ch. 3) that to let this lapse entirely is to fall into a jejune kind of Quinean across-the-board empiricism—the phrase ‘in us’ seems to demand an (idealist) justification of objective validity, or else to leave us with skepticism. But if the reading of Wittgenstein I’ve been pushing is right, he is, precisely, guided by a vision according to which the alternatives “content of experience” and “in us” aren’t exhaustive: compare the discussion of *Zettel* §357 at the end of §5 c) ii) above.

³⁰ After all, even a transcendental deduction must start somewhere.

³¹ This locution comes from personal communication with Rödl.

³² I don’t mean to trivialise this: obviously work is required to explain this distinction (and Rödl’s claim is precisely that his conception will result from such work, if it is rightly done). I mean that the reading of Wittgenstein I have been urging at least purports to contain resources to reconstruct a parallel distinction: notably, in the distinction between factual and grammatical remarks.

Now, Rödl motivates his demand for a justification on the basis of some principle with the observation that the status of the “grammatical knowledge” (as Rödl calls it) explored in the *Investigations* is synthetic *a priori*:

[Wittgenstein’s] grammatical remarks have all the traditional attributes of logical knowledge. They are pure—they do not describe sensuous experience; they hold (if they hold) *a priori*—they can be neither proven empirically nor disproved empirically; and they are synthetic—they do not hold on the basis of arbitrary meaning assignments. (2005: 9)

And as we have seen, Rödl moves from the fact that Wittgenstein seems “to want to reject... the very idea of a general form of thinking” to the claim that:

... an obscurity is cast over that strand of the analytic tradition which arises from the *Philosophical investigations*, an obscurity over the nature and possibility of ‘grammar’ and thus over the object of philosophical investigation. (2005: 10)

But the whole thrust of the chapter so far has been to show how reflection “from within” on the structures of our practices—that is, reflection that progresses piecemeal, on different practices, without beginning from some one principle or some “idea of a general form of thinking”—is nevertheless a *conceptual*, not (say) a merely *empirical*, exploration; and, more to the point, that the concepts at issue are in some sense *necessary* concepts.

I realise that I haven’t put entirely to rest Rödl’s complaint against Wittgenstein. But I have tried to bring out the difficulty of adjudicating the dispute, and I have suggested that, when we appreciate this difficulty, we might come to find Rödl’s demand for further justification less pressing. In any case, I hope I have said enough to justify looking for an understanding of the origin of the *Investigations*’s “grammatical remarks” alternative to Rödl’s claim that they are implicit (and as yet unjustified) appeals to transcendental logic, awaiting the development of a system of grammar from a principle to serve as their foundation. It is to the task of looking for such an alternative understanding, then, that I turn in the next chapter.

Chapter Five. From Frege and Russell to the *Tractatus* and beyond.

Our task in this chapter is to account for Wittgenstein's development from the views articulated in the *Tractatus logico-philosophicus*, as they arose out of engagement with those of Frege and Russell, in the direction of the ones in *Philosophical investigations* discussed in the previous chapter. For we argued there that Rödl's doubly contentious interpretative claim about the *Investigations*—to the effect that it consists in transcendental-logical investigations but that, failing to recognise this in itself, it owes us a justification of its categories—fails to do justice to Wittgenstein's claim that the search for such foundations is misplaced. Rödl's claims appeared to presuppose an exhaustive dichotomy between Frege's deductive and Kant's transcendental conceptions of logical form: it was on this basis that he could hold that, if Wittgenstein after the *Tractatus* abandoned the Fregean conception, but retained, however unreflectively, the project of exploring logical form in *some* sense, that sense must be the transcendental one. The previous chapter gave reasons for thinking that what Wittgenstein is doing in the *Investigations* is, anyway, not meant to be transcendental logic, at least not if this ascription brings with it, as Rödl implies, the need for a deduction of the validity of its categories. But it might well be asked why the enquiries of the *Investigations* should be conceived as “logical”, as consisting in explorations of “logical form” in any sense, at all. The present chapter will therefore address this question, through a historical approach. If we can see the *Investigations* as engaged in an enquiry recognisable as the descendant of Wittgenstein's engagement in the *Tractatus* with Frege's and

Russell's manifestly logical works, we shall have some grounds for holding that the former do not represent a mere change of subject, but rather a change in *conception* of the subject matter, mandated, as it were, by reflection on that subject matter itself.

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1. Frege on quantification

It has often been noted that Frege's treatment of the truth functions and quantifiers is as particular (though quite abstract) *contents*, in some tension with the commonplace that logic is a purely *formal* discipline.¹ Thus conjunction, for instance, is on his account a particular function from pairs of objects to objects (in particular "truth values", though for Frege this is only to specify further its content, not its logical type), while the first-order universal quantifier is a second-level function from first-level functions (that is, functions from and into the set of objects) into the set of objects (also always having as its value a truth value).

¹ MacFarlane (2000) constitutes an admirable study of this constellation of notions. Somewhat oddly to my ear, he understands Frege's conception of logic as a discipline which "provides constitutive norms for thought as such" as a perfectly intelligible thing to mean by 'formal', indeed making it one of the three competing conceptions of the formality of logic among which he adjudicates. It strikes me as odd, though, as I say, to call a conception one of *formality* if (as MacFarlane recognises explicitly) it does not involve a contrast between form and *content* or *matter*.

Now, it is famously a guiding thought of Wittgenstein's *Tractatus logico-philosophicus* that this treatment of logical notions as particular contents cannot be right.² Thus, as the *Tractatus*'s proposition 4.0312 tells us: "My fundamental thought is that the 'logical constants' do not represent." The fact that the truth functions are interdefinable—which Frege already recognised³—leads Wittgenstein to conclude that a sign for a truth function in a sentence does not characterise the sense of that sentence. (For if we can express a given proposition indifferently as ' $\sim(A \vee B)$ ', ' $\sim A \ \& \ \sim B$ ', or ' $A \downarrow B$ ', then none of ' \sim ', ' \vee ', ' $\&$ ' or ' \downarrow ' is essential to the expression of, and hence to the sense of, the proposition.) What is essential is the one truth function that each of these combinations of symbols expresses: that is, the truth function that is expressed in the truth tables for all of these sentences. But then, Wittgenstein argues, the task of logic must not be the study of the various truth-functional "logical constants" which can (but need not) be used to express those truth functions, but rather the study of the way in which propositions can be built truth-functionally out of elementary propositions.

Something similar happens to quantification. Wittgenstein "separate[s] the concept *all* from the truth-function" (§5.521); but as in the case of the truth-functional "logical constants", he rejects the Fregean approach to the quantifiers according to which a quantifier is treated as a particular function from concepts to truth values. Let us consider Frege's approach a bit more closely, in order to appreciate better Wittgenstein's rejection of it.

² The remarks in the text to follow treat the content of the doctrine of the *Tractatus*. But is what's presented in the *Tractatus* a doctrine? Not if we're meant to take seriously, as we surely are, the claim that "he who understands me finally recognizes [my propositions] as nonsensical" (§6.54) (not to mention the string of remarks on the nature of philosophy found in the comments on §4.11). Still, as we read the book, we are surely invited to think through the propositions as we would those of a doctrine; here I shall therefore, reasonably I think, abstract from the book's puzzling frame.

³ as we mentioned briefly in Chapter II. See e.g. Frege (1880-81: 37): "Now to obtain a sign joining two contents of possible judgment whose meaning was as simple as possible, I had four choices open, all from this point of view equally justified: I could have adopted as the meaning of such a sign the denial of any one of the four [combinations of affirmation and denial of the two contents]. But it suffices to choose one, since the four cases can be converted into one another...."

A list of the symbolic apparatus of Frege's *begriffsschrift* might lead one to expect that Frege's explanation of his symbols would include a single, univocal treatment of the "concavity" he uses to express generality. But in fact in *Grundgesetze* he introduces the concavity first in its application only to generality over objects (§8). Part 1.iv) of that work is titled "Extension of the notation for generality," and its first section, §19, indeed makes use of what appears to be the same concavity (with, indeed, some of the same rules for its use, *e.g.* the rule for determining what the "corresponding function" is to which it is applied in a given case). But his explanation of the *Bedeutung* of an instance of the use of the concavity to express generality over first-level functions is given entirely independently of the analogous explanation for generality over objects back at §8. And this is no coincidence. We discussed in Chapter II the pressure on Frege to understand his symbolism function-analytically. But on such principles, the distinction between generality over objects and generality over first-level functions, cashed out as it is in terms of the distinction between second-level and third-level functions, is (to adapt slightly the words from his paper "Function and concept" we quoted in that chapter) "not made arbitrarily, but is founded deep in the nature of things."

But the following words from the *Tractatus* (although its author illustrates them using a different example) contain the material for an objection to just this approach to generality:

If logic has primitive ideas these must be independent of one another. If a primitive idea is introduced it must be introduced in all contexts in which it occurs at all. One cannot therefore introduce it for *one* context and then again for another.... for it would then remain doubtful whether its meaning in the two cases was the same, and there would be no reason to use the same way of symbolizing in the two cases. (§5.451)

One might see Frege's repeated use of the concavity, and some of his patter surrounding it, as indicative of a conception of a unitary notion of *generality* as a primitive idea of logic. But this is then betrayed by the fact that the notation needs to be reintroduced, and given a fresh

explanation, for each level over which it is to be used to range. One way of understanding the treatment of generality in the *Tractatus*—and of putting in some context the account of “forms of object” in the book’s opening pages—is to recognise it as an attempt to give a genuinely unitary account of generality, in contrast to Frege’s.

There are remarks about generality scattered through the *Tractatus*, but it is most instructive to consider the rôle it is meant to play in “the general form of proposition”, namely “[$\bar{p}, \bar{\xi}, N(\bar{\xi})$]” (§6), which “says nothing else than that every proposition is the result of successive applications of the operation $N(\bar{\xi})$ to the elementary propositions” (§6.001).⁴ “ $N(\bar{\xi})$ is the negation of all of the values of the propositional variable ξ ” (§5.502); in other words, it is a generalisation of the NOR operator in terms of which, just as in terms of the Sheffer stroke (NAND), Sheffer showed it possible to define all of the truth functions. But if N is in this way truth-functional (and \bar{p} represents the totality of elementary propositions), where does generality come into this purported representation of “that which *all* propositions, according to their nature, have in common with one another” (§5.47)? In the notation of §6, it comes in through the variable ξ . About this, Wittgenstein tells us:

The values of the variable are stipulated.

The stipulation is a description of the propositions for which the variable stands.

How the description of [these propositions] takes place is inessential.

We *may* distinguish three kinds of description: 1. Direct enumeration. In this case we can simply give its constant values instead of the variable. 2. Giving a function fx whose values for all values of x are the propositions to be described. 3. Giving a formal law according to which those propositions are constructed. In this case the [propositions] are all the terms of a formal series. (§5.501)

The notation of §6, in other words, has the following import: to construct any proposition whatever, we are to begin with the elementary propositions, take a selection of them (by whatever means we wish) and jointly negate that selection; then, if we wish, we may repeat the

⁴ I have derived much help in my understanding of Wittgenstein’s account of generality in the *Tractatus* from an unpublished paper by Thomas Ricketts, “Logical segmentation and generality in Wittgenstein’s *Tractatus*.”

procedure, now starting with the elementary propositions together with the result of the previous application of the operation. Generality comes in through the *selection*: for we may, for instance, begin by selecting *all* the elementary propositions; or we may select all the elementary propositions containing a given expression; or again, we may select all the propositions that result from repeated application of a given operation to some beginning proposition.

What should be striking about this account of generality, especially against the background of the Fregean approach we sketched a few paragraphs back, is that it makes no essential reference to the structure of the propositions providing the basis for a given generalisation. Whereas Frege's principles lead him to understand all propositional articulation in function-analytical terms, so that he must explain generality over objects separately from generality over functions (and indeed separately for each level of function)—that is to say, must explain generality by making explicit reference to the other aspects of the structure of the propositions in which it is involved—Wittgenstein “locates” generality once and for all in the selection of propositions for joint negation, however that selection is carried out. Indeed, he tells us explicitly, as we just quoted, that the procedure involved in this selection is “inessential” (§5.501). He goes on, we saw in the same section, to give examples of how it may go—examples which reveal that the “general form of proposition” can indeed encompass (at least some of) the propositions which Frege and Russell would express using quantifiers—but he emphasises that it is not essential to an account of generality to spell these examples out: “We *may* distinguish three kinds of description,”⁵ but it is not incumbent upon the logician to do so; and by the way, nothing is said to suggest that there aren't other kinds besides these three.

⁵ Ogden's translation omits the emphasis on ‘*können*’, but it is clear in the German.

And this helps explain the fact that the book appears to lack the kind of attention to categorial analysis that Frege's work might have led us to expect. Atomic facts, we are told, consist of "combination[s] of objects" (§2.01) (rather than, say, combinations of objects *with functions taking those objects as arguments*⁶). We are also told that these objects "contain the possibility of" the states of affairs in which they can occur, the range of such possibilities being "the form of the object" (§§2.014f.); this is perhaps suggestive of categorial distinctions such as Frege's (for we may think of first-level one-place functions, for example, as "objects" which can occur together with (Fregean) objects to form facts; second-level functions as "objects" which may combine with first-level functions; and so on). But very little is said definitively⁷ about the particular forms of object there are: examples such as those I just gave are absent. And all this is recapitulated at the level of propositions. An elementary proposition is "a connexion, a concatenation, of names" (§4.22); while the ranges of possibilities of combination of simple names with one another in elementary propositions are isomorphic with the forms of the objects for which they go proxy, we are again given no details about these ranges. We are not given a distinction between singular term and predicate, any more than between object and function.

It is important to the Tractarian account that there *is* structure in atomic facts and in elementary propositions. That "the proposition is articulate" (§3.141), and the related notions that the picture is a fact (§2.141) and that the fact is a connection of objects (§2.01), is a leitmotiv of the book. It has particular relevance to the present discussion, in so far as generality, as opposed to the "direct enumeration" of propositions that results in a straightforward, non-general truth

⁶ I don't mean to suggest that the parenthetical alternative is a gloss of Frege's view; on the contrary, Frege's extensionalism leads him, famously, to hold that the reference of a thought is a truth value. The realm of reference, though it does contain objects and functions, does not contain "facts" composed of them.

⁷ I phrase this a little weakly because Wittgenstein *does* give us a handful of examples, but they are problematic. I return to the discussion of his examples below.

function of elementary propositions, is bound to proceed, by one or another means, through the collection of propositions on the basis of something they have in common with one another; and that two distinct symbols have something in common will be true in virtue of their being *composite* (§5.5261). But, again, while the *fact* of elementary propositional articulation is important to the account of generality in the *Tractatus*, the particular *nature* of this articulation is held not to be a matter of interest to logic. This is made explicit in the stretch of text beginning with §5.55:

[W]e cannot give the composition of the elementary proposition....
 The enumeration of any special forms would be entirely arbitrary....
 It is clear that we have a concept of the elementary proposition apart from its special logical form.
 Where, however, we can build symbols according to a system, there this system is the logically important thing and not the single symbols. (§§5.55, 5.554, 5.555)

2. Russell on “logical forms”

We can arrive at something like the same view of the rôle of a categorial analysis in the *Tractatus* if we consider its evolution from an engagement with the views of Bertrand Russell. Though it was fashionable among some *Tractatus* scholars in the latter half of the twentieth century to downplay the influence of Russell on Wittgenstein’s *Tractatus* in favour of that of the “great works of Frege,” there is no doubt that many of the problems Wittgenstein wrestled with during his writing of the *Tractatus* arose from Russell’s treatment of similar problems. I have in mind, as particularly relevant to the present study, the issue of *logical forms*, as it arose in Russell’s struggles during the early nineteen-teens over the nature of propositions. Historians of Russell’s and Wittgenstein’s thought such as David Pears, Brian McGuinness, Peter Hylton and Thomas Ricketts⁸ have told much of this story admirably well, so I shall confine myself to a brief outline (credit for the content of which, indeed, is due in large part to them, in particular to Hylton).

⁸ Pears (1981); McGuinness (1974); Hylton (1984, 1990); and Ricketts (1996).

At the turn of the twentieth century, G.E. Moore⁹, and Russell following him¹⁰, found themselves resisting the subjectivism they thought they perceived in idealism, by (in particular, for our purposes) understanding propositions not as mind-dependent syntheses of elements, but rather as mind-independent, objective furniture of the world. However, the account of judgment which suited this conception—namely, as a binary relation between the judging subject and the proposition judged¹¹—made no reference to the structure of the propositions in question; and likewise, there appeared to be no room in such a conception of propositions for an account of *truth* besides as a brute, inexplicable property holding of some propositions and not of others, propositions which were otherwise on an ontological par. This consequence was in fact embraced explicitly by both Moore and Russell.

Russell's development of his "multiple relation theory of judgment" (1906-1913)¹² was a result of his having come to realise the inadequacy of the accounts of judgment and truth resulting from this conception of propositions. The multiple relation theory in fact dispensed with propositions altogether as basic ontological elements, in favour of the acts of judgment on the part of judging subjects. These acts of judgment (or, more generally, of any "propositional attitude") are understood, in the first version of the theory, as relations in which the judging subject stands at

⁹ "The nature of judgment" (1898).

¹⁰ See e.g. *Principles of mathematics* (1903); Hylton also provides some delightful quotations from Russell's 1904 paper "Meinong's theory of complexes and assumptions."

¹¹ This may not seem quite to capture the structure of Moore's theory, in which "concepts" are more fundamental than "propositions." But "a proposition is nothing other than a complex concept" (§12), and though the account of propositions' truth and falsity makes *some* reference to their composition out of concepts, it is entirely schematic: "A proposition is constituted by any number of concepts, together with a specific relation between them; and according to the nature of this relation the proposition may be either true or false. What kind of relation makes a proposition true, what false, cannot be further defined, but must be immediately recognised." So I think the simplification in the text is harmless. As for Russell, on the one hand *Principles of mathematics* Ch. IV is an enquiry into the "philosophical grammar" of the proposition; but on the other hand, Russell there makes it explicit that propositions are no less *terms* than their constituents. It is Russell himself who later describes his view from this period as a dyadic theory of judgment.

¹² The earlier version of the theory is expounded in Russell's (1910); the later version is given in the 1913 manuscript *Theory of knowledge*.

once to (what would, before the multiple relation theory, have been called) the elements of the proposition. Russell next modified the theory of judgment to include, as one of the relata involved in a propositional attitude, the “logical form” itself. Russell explains that mere entertainment of (what would hitherto have been called) the elements of the proposition does not suffice for its understanding; one must also know how they are to be put together.¹³ As he explains, “when *all* the constituents of a complex have been enumerated, there remains something which may be called the ‘form’ of the complex, which is how the constituents are combined in the complex” (1913: 98). Such forms are “logical objects” acquaintance with which, as we just saw, forms a part of an act of judgment—but “[i]t would seem that logical objects cannot be regarded as ‘entities’” (1913: 97): “the form is not a ‘thing’, not another constituent along with the objects that were previously related in that form” (1913: 98). Russell suggests that when we existentially generalise on every contentful element of a proposition, we arrive at its form: thus

... ‘something has some relation to something’ contains no constituent at all. It is, therefore, suitable to serve as the “form” of dual complexes. In a sense, it is simple, since it cannot be analyzed. At first sight, it seems to have a structure, and therefore to be not simple; but it is more correct to say that it *is* a structure. (1913: 114)

Nevertheless, $(\exists x)(\exists R)(\exists y)xRy$ is also a judgment; indeed a true one.

However, though the multiple relation theory brought back into focus the constitution of a proposition out of elements, in contrast with Moore’s and Russell’s earlier view, it recognised no restrictions on the range of collections of elements so unifiable into propositions—or, in Wittgenstein’s words, it did not “show that it is impossible to judge a nonsense” (*Tractatus* §5.5422). The modification of the theory to include “logical forms” as relata in acts of judgment

¹³ I am omitting from this summary the way Russell, in earlier versions of the theory, tried variously to appeal to what he called the “sense” of the relating relation in the judgment and to the order of the relata in order to settle the question how the elements are to be put together in the judgment. Ricketts (1996) is helpful on this.

does not help it to avoid this objection¹⁴, for no mechanism is provided to ensure that the other relata involved in a given judgment actually conform to the requirements of the “logical form.” That is, Russell gives no account of how the presence of a “logical form” as one of the relata involved in a propositional attitude places constraints on the other relata. And it is very difficult to see how such an account could go, in view of, on the one hand, the apparently incoherent and in any case vague account of “logical forms” themselves, and on the other hand the fact that, given that the point of the multiple relation theory was to account for propositions in terms of more basic propositional attitudes in order to avoid the pitfalls of the earlier anti-idealist theories of propositions as basic entities, Russell has prevented himself from making reference to the nature of propositions in an account of the possible combinations of relata available to judgment.

Now, it is clear, for instance, that Wittgenstein is engaging with just these puzzles when he writes these words (from which we quoted above):

The correct explanation of the form of the proposition “A judges *p*” must show that it is impossible to judge a nonsense. (Russell’s theory does not satisfy this condition.) (§5.5422.)

But when one looks for them, one can find hints of this engagement in the early part of the *Tractatus* as well. Indeed, a way of expressing what Wittgenstein is doing in these opening pages is to say: he is locating *form* in the *objects themselves*—in their possibilities of combination—rather than in mysterious further objects serving (on a plane with particulars, properties, relations etc.) as relata involved in acts of judgment, belief and so on. The following *Tractatus* propositions, for instance, can be seen to speak directly to Russell’s puzzles:

It is essential to a thing that it can be a constituent part of an atomic fact. (§2.011)
 If I know an object, then I also know all the possibilities of its occurrence in atomic facts. (§2.0123)
 The possibility of its occurrence in atomic facts is the form of the object. (§2.0141)

¹⁴ Indeed, as several commentators note, Wittgenstein apparently made his objection upon reading the 1913 manuscript, which contains the version of the multiple relation theory including “logical forms”.

And in a passage from an early notebook, Wittgenstein gives this approach clear expression:

The logical form of the proposition must already be given by the forms of its component parts....
In the form of the subject and of the predicate there already lies the possibility of the subject-predicate proposition, etc.... (*Notebooks*, p. 23)

It is clear that, having in place this conception of objects as carrying with them the range of possibilities of combination with other objects, the picture theory is designed to place the sort of constraint on the possible objects of judgment of whose absence from Russell's account §5.5422 complains. For since (in the elementary case¹⁵) we entertain propositions by "mak[ing] to ourselves pictures of facts" (§2.1), but pictures consist of elements going proxy for objects with which they share the same ranges of possibilities of combination, "[w]e cannot think anything unlogical" (§3.03). To stand in some kind of cognitive relation to a set of elements in which the possibility of combination one with another does not lie is, anyway, not to *picture* anything: and that is, not to think (§3).¹⁶

For my purposes here, the point of rehearsing this history of Wittgenstein's engagement with Russell's work of the nineteen-teens has been to bring out, from another direction, the importance of the following aspect of the Tractarian approach. Where Russell takes himself to be obliged to provide an account of the logical categories, as well as an account of the logical forms in which elements of those categories are unified into propositions, the *Tractatus* aims to provide an account of propositions sufficient for the purposes of logical theory without having to enter into the details of the types of object or of combination at all. And again, a recurring theme

¹⁵ The case of non-elementary propositions is more complicated to describe; but since my goal here is only to show how Wittgenstein is engaging with Russell's problems about "logical forms," and those problems are already patent at the elementary level, it suffices for us to confine our attention to this level.

¹⁶ Indeed, it would not be too strong to say that, in such a case, one is not "in some kind of cognitive relation" with meaningful elements at all. The import of the *Tractatus*'s version of Frege's "context principle" (§3.3) and its ensuing discussion, including the distinction between symbols and mere signs (see especially §§3.32ff.), secures this result. Cf. Diamond (1981). (However, a distinction such as this one is not available in Russell's framework, since the relata involved in Russell's account of judgment correspond more closely to the *Tractatus*'s "objects" than to its symbols.)

of the *Tractatus* is the importance of that which is most *general*, which makes the more specific possible.

Indeed, the parallel between “logical forms” and the truth-functional “logical constants” is close indeed. As a matter of fact, Russell in the *Theory of knowledge* manuscript treats his “logical forms” as of a piece with the truth functions and quantifiers, under the heading of “logical constants”; he supposes that analogous problems are associated with the epistemological status of all of these. Wittgenstein’s claim that logic needn’t and hence oughtn’t treat the “logical constants” as separate special contents is more explicit in the case of the truth functions; but we can see in the light of the story I’ve just rehearsed that he holds the same view of Russell’s “logical forms.”¹⁷ Wittgenstein’s view is that, as soon as we have available an account of elementary propositions that explains their fitness to be truth-bearers, we thereby also secure, at once, the whole of the truth-functional and quantificational apparatus for them. (Compare §5.442, though similar ideas are expressed frequently in the vicinity.) Necessary for this, Wittgenstein takes it, is the picture theory. The picture theory explains why truth and falsity are (unlike on Moore’s and Russell’s early view) not brute, inexplicable properties of propositions.

(Here compare §6.111:

One could *e.g.* believe that the words ‘true’ and ‘false’ signify two properties among other properties, and then it would appear as a remarkable fact that every proposition possesses one of these properties. This now by no means appears self-evident, no more so than the proposition ‘All roses are either yellow or red’ would sound even if it were true.

The example of the roses echoes explicitly a paper of Russell’s from 1904, in which he embraces just this consequence of his own view. In this connection it is worth remarking that Moore’s and Russell’s account of truth from that period made the possibility of an account of the inferential

¹⁷ McGuinness makes this clear in his (1974).

relations in which propositions stand to one another also utterly obscure; from this angle, too, we can see how Wittgenstein takes it that a correct account of truth will bring an account of logic with it.)

The picture theory also provides an account of the impossibility of nonsensical thought, as we have seen. But the picture theory can be articulated without entering into details about the nature of the categories of object—without, that is, a classification of the various ranges of possibility of combination into which objects can enter one with another—and likewise without giving a specific account of the forms of elementary proposition. In this way, again, the *Tractatus* presents a conception of logic according to which, though the very *fact* of categorial structure is essential (for essential to the picture theory is the idea that facts, and pictures, and propositions, are *articulate*), it is beyond the purview of logic to give the *details* of that categorial structure. (Cf. §5.5571: “If I cannot give elementary propositions *a priori* then it must lead to obvious nonsense to try to give them.”) And as we saw in Chapter II, this is sharply opposed to Frege’s conception of propositional articulation: for Frege’s view was that to give an account of the inferential behaviour of propositions, elementary or not, is at once to give an account of their internal structure—and that is to say, a *substantive* account, not merely a reference to whatever structure they might have.

3. Forms of object and configurations of object

The difference I am pointing to between Frege and Russell on the one hand and Wittgenstein on the other *vis-à-vis* the categorial analysis presupposed by logic is more than a simple matter of the difference between spelling the analysis out explicitly and leaving it—that very same analysis—unsaid. For it is a real question whether the categorial analysis alluded to so abstractly

in the opening pages of the *Tractatus* is indeed the same as Frege's (or Russell's¹⁸) at all. Indeed, there is a long tradition of esteemed commentators on the *Tractatus* who have maintained that we are given enough information about that categorial analysis—that is (to put it clumsily), about the range of objects' ranges of possibilities of combination with one another—to know that it cannot be Frege's or Russell's. I have in mind the claim, to be found in work of Anscombe, Sellars, Ishiguro, and Ricketts¹⁹ (among others), that (to a first approximation) the “objects” of the *Tractatus* are particulars—that is, more or less, what Frege too called objects—and the “configurations” of these objects that constitute atomic facts (§2.0272) correspond to (some of) Frege's concepts. If the author of the *Tractatus* is merely *abstracting from* (by which I mean, in this case, refraining from stating, while all along presupposing) a categorial analysis along the lines of Frege's or Russell's, then this claim cannot be correct. For to do *this* would be to use ‘object’ to range over Fregean objects, first-level functions (of one place, two places, etc.), second-level functions (*ditto*), and so on—covering the differences among these categories under the catch-all idea “form of the object” (§2.0141), that is, “[t]he possibility of its occurrence in atomic facts.” But the readings advanced by Anscombe, Sellars, Ishiguro, and Ricketts depart radically from this: for on their readings, the “form of an object” is (in nonTractarian terminology) the range of *predicates* (including relations with other objects) an object admits, while predicates themselves vanish entirely from the ontological scene. As the place of

¹⁸ There are significant differences between the categorial structure with which Frege works and that treated by Russell. For example, Hylton has a very helpful paper (1994) discussing the ramifications of the fact that Russell, unlike Frege, takes the idea of *propositional* functions, and not of functions generally, as primitive. (And in his (1997), Hylton argues that we can make good sense of many puzzling passages of the *Tractatus* if we understand Wittgenstein's use of ‘function’ as of a piece with Russell's rather than with Frege's.) Still, for the purposes of the present discussion, I think we can abstract from these important differences between Frege and Russell, and see that the concerns motivating Wittgenstein arise equally from both Frege's and Russell's conceptions of structure.

¹⁹ Anscombe (1959); Sellars (1962); Ishiguro (1969); Ricketts (1996). Ricketts has told me that he no longer subscribes to the claim in question.

categorial analysis in the *Tractatus* is central to our concerns in this enquiry, it is worth pausing to assess this claim.

For the sake of having a pair of labels, I shall call readings such as Anscombe's, Sellars's, Ishiguro's, and Ricketts's "particularist", and any reading according to which Tractarian "objects" include properties, relations and so on "Russellian". (I use 'Russellian' with some reluctance, as I don't wish to communicate the impression that, were the reading I'm calling "Russellian" correct, it would imply that Wittgenstein's conception of categorial structure is entirely indistinguishable from Russell's. On the contrary, on any reading, there will be differences between Wittgenstein and Russell (and Frege) *vis-à-vis* the place of categorial analysis in logic. Still, the point of the label is to remind us that, however we're to *account* for the categorial analysis, anyway *what categories it consists in* lines up more or less with the categories employed by Russell (or Frege).²⁰)

- a) Objects as particulars, forms as universals: the "particularist" reading
 - i. Sellars

Wilfrid Sellars (1962)—although his article is centred around the question whether we should think of Tractarian objects as only particulars or else as including also universals—takes §3.1432 as incontrovertible evidence for the view that Wittgenstein's account, anyway, is the former.

§3.1432 reads as follows:

Not: "The complex sign 'aRb' says that a stands in the relation R to b," but rather: *that* 'a' stands in a certain relation to 'b' says *that* aRb.

²⁰ In view of footnote 18, and since I shall tend in what follows to speak of functions generally rather than of propositional functions, the label 'Fregean' might have been better than 'Russellian'. However, I will frequently use 'Fregeanism' as a label for the view discussed in Chapter II of the relation between categorial analysis and the systematisation of inference. For the sake of disambiguation, then, I'll use 'Russellian' for the reading of the *Tractatus* now under discussion.

Sellars does not offer a long *argument* for his reading of the *Tractatus*; he simply quotes §3.1432, and tells us what it means:

Now in Frege's system, 'R' would be said to stand for (*bedeuten*) a concept, whereas 'a' and 'b' stand for objects. Thus what Wittgenstein puts by saying that configurations of objects are represented by configurations of names (3.21)... could also be put by saying that to represent that certain objects satisfy an n-adic concept, one makes their names satisfy an n-adic concept. Roughly, Wittgenstein's configurations are the counterparts of a sub-set of Frege's concepts, and Wittgenstein is taking issue with Frege by insisting that a perspicuous language would contain no concept words functioning predicatively.... (1962: 227)

Sellars discusses at length a language, which he calls "Jumblese" (and which we touched on in our discussion of the functional structure implicit in a *begriffsschrift* in Chapter II), which adheres strictly to this principle: thus, for instance, one might express that a certain object is green by writing its name in green ink, or express that one object is beside another by writing their names beside each other. (Note that his claim is not that the linguistic expression of the instantiation of a concept must recapitulate precisely the content of the concept itself, as in these two examples; on the contrary, Sellars's own favoured example is the expression of the fact that an object is green not by writing its name in green ink but by writing it in boldface.) I'll return later to some interesting consequences of Sellars's discussion of this language; for now, I just want to record that Sellars takes it as unequivocal that this yields a perspicuous representation of Wittgenstein's account of atomic facts and the elementary propositions that picture them, and that Sellars's basis for holding this is (nothing more than) §3.1432.

ii. Anscombe

Elizabeth Anscombe also rejects the view that Tractarian objects might include indifferently particulars and universals, or Fregean functions; as she writes:

What... has become of Frege's 'concepts' in Wittgenstein's theory? They seem to have disappeared entirely; actually, however, instead of making concepts or universals into a kind of objects..., Wittgenstein made the gulf between concepts and objects much greater than Frege ever made it.... [I]n respect of having argument-places, concepts go over entirely into logical forms. In the 'completely analysed proposition', which is 'a

logical network sprinkled with names', the Fregean 'concept', the thing with holes in it, has become simply the logical form. (1959: 108-109)

Now, her view isn't exactly Sellars's: for, while Sellars, as we saw, takes §3.1432 at face value, and jumps into a discussion of the proper understanding of 'aRb' without any preliminaries, Anscombe carefully adds the qualification: "*in the 'completely analysed proposition.'*" Thus, for instance, while Sellars is happy to treat 'a is green' as a proposition whose perspicuous, Jumblese (and hence Tractarian) presentation would involve just one name, 'a', Anscombe in contrast recalls the discussion of colour at §6.3751, which implies that colour ascriptions are not elementary propositions, and hence puts her point as follows:

... for Wittgenstein the two facts: A is red, and: B is red, would be analysed into (1) facts corresponding to the descriptions of the complexes A and B, and (2) facts about the elements of the complex A along with certain further elements, say a, b, c, for A's redness, and exactly corresponding facts about the elements of the complex B along with certain other elements, say d, e, f, for B's redness. There is no need for a, b, c, to be the same as d, e, f, respectively; for it is only the 'logical network' that is 'universal'. (1959: 110)

Thus Anscombe does not claim that just *anything* Frege would regard even as a first-level concept is to be understood as a configuration of simple objects. (And this refinement of Sellars's view will become important for us later.) But it is clear that Anscombe is still in the same camp as Sellars with respect to our question. For what plays a predicative rôle in the propositions with which analysis terminates is the configuration. And in case our first quotation from Anscombe doesn't make this clear enough, she says explicitly that properties and relations cannot be included among Tractarian objects, adding as argument the observation that "if Wittgenstein held that objects fell into such radically distinct categories as functions and individuals, it is an incredible omission not to have made this clear" (1959: 109n1).

iii. Ricketts

More recently, Thomas Ricketts has given an exposition of the picture theory of the *Tractatus* according to which the ways objects hang together in the atomic fact constitute as it were

material properties of those objects: thus he works with the example ‘a envies b’, and, like Sellars, explains how the *Tractatus* treatment of such a proposition would find in it just the two names ‘a’ and ‘b’, “hanging together” in the proposition in such a way as to represent that the two objects named “hang together” in a certain way, namely, that of envying. This hanging together, this configuration, is, as Ricketts flags, “not ‘purely formal’” (1996: 76). Moreover, like Anscombe, he is sensitive to the importance of the assumption that the proposition in question is elementary; indeed, parallel to Anscombe’s discussion of colour ascriptions, Ricketts argues that the logical structure of ascriptions of spatial relations such as *to the right of* show that they cannot be elementary. Ricketts motivates his reading of the *Tractatus* primarily through a rich account of its development from Wittgenstein’s unpublished 1913 manuscript “Notes on logic,” and a discussion of what certain *Tractatus* propositions come to on his reading: not through outright argument against the “Russellian” alternative, according to which (at least some) properties and relations, too, are Tractarian “objects”. But he gives a quick, suggestive argument in a footnote. In response to the suggestion that a given elementary two-place relational proposition should not be understood as consisting in “two things connected by a relation”, but rather simply as three objects hanging together as in a chain, Ricketts responds:

This is a peculiar remark. If an atomic fact is not two objects connected by a relation, then there seems to be no ground for calling any constituent thing in it a relation.... This view of relations is utterly unlike either Russell’s or Frege’s. (1996: 98n32.)

iv. Wittgenstein’s illustrations

Finally, Wittgenstein’s own illustrations of his notion of the form of an object seem to offer some support for the “particularist” reading. Consider:

A spatial object must lie in infinite space.... A speck in a visual field need not be red, but it must have a colour; it has, so to speak, a colour space round it. A tone must have a pitch, the object of the sense of touch a hardness, etc. (§2.0131)
Space, time and colour (colouredness) are forms of objects. (§2.0251)

Although it is conceivable that we are not to hold Wittgenstein to these particular examples—and his discussion of colour ascriptions at §6.3751, which (as we saw in connection with Anscombe) seems to imply that they cannot be elementary, renders the colour examples here rather hard to understand—still, it would be odd if these examples were all wildly misleading. But if the “Russellian” reading is right, then the forms of object should be such as: “particular”, “property”, “two-place relation of particulars” and so on—quite different from, and quite a lot *thinner* than, anything Wittgenstein adduces in this vicinity as an illustration of his meaning.

b) The “Russellian” reading

Nevertheless, advocates of the “Russellian” reading have shown how good sense can be made, after all, of much of the material in the opening pages of the *Tractatus* even on such a “thin” account of forms of object and of configuration (and hence of forms of elementary proposition). As we have already seen, historians of Wittgenstein’s thought such as David Pears, Brian McGuinness, Peter Hylton and indeed Ricketts have shown how much of it can be seen to emerge from his engagement with Russell in the early nineteen-teens, and in particular with the problems which prompt Russell to keep changing his mind about the nature of propositions and their form from about 1910-1913. The examples of space, time, colour and tones remain puzzling. But it should be noted that there are other places in the book where Wittgenstein adduces examples that line up perfectly with the “Russellian” reading. Thus:

The elementary proposition consists of names. Since we cannot give the number of names with different meanings, we cannot give the composition of the elementary proposition. (§5.55)
Russell said that there were simple relations between different numbers of things (individuals). But between what numbers? And how should this be decided—by experience? (§5.553)
The enumeration of any special forms would be entirely arbitrary. (§5.554)
How could we decide a priori whether, for example, I can get into a situation in which I need to symbolize with a sign of a 27-termed relation? (§5.5541)

The last quoted sentence here gives a concrete, and fully “Russellian”, example of a form. Indeed Russell is mentioned in this stretch of text by name. And if the sort of forms Wittgenstein were imagining were not “Russellian” but rather “particularist”, it is hard to see how the reasoning contained in the first paragraph quoted here would even join the question what elementary forms there are.

And finally, there is, in fact, a good deal of evidence for the claim that, for much of the time Wittgenstein is doing his preliminary work for the *Tractatus*, and indeed writing it up, he has in mind what we have been calling “Russellian” forms. A natural place to look for evidence that would help to resolve this dispute would be in the notebooks Wittgenstein kept, and letters he wrote, while he worked on what would become the *Tractatus*, as well as at the records his students have kept of the remarks about the *Tractatus* he made after its publication. And this sort of evidence seems to provide nearly unequivocal support for the “Russellian” reading. It is true that, in a letter to Russell from the beginning of 1913, he wrote:

I have changed my views on “atomic” complexes: I now think that qualities, relations (like love) etc. are all copulae! That means I for instance analyse a subject-predicate proposition, say, “Socrates is human” into “Socrates” and “something is human”, (which I think is not complex). The reason for this is a very fundamental one: I think that there cannot be different types of things! (1914-1916: 120-121 (16.1.13))

And we find what appear to be echoes of this thought—antecedents of the proposition §3.1432, of which we saw Sellars make so much—in his two sets of “Notes on logic” from 1913 and early 1914. Indeed, Ricketts, in his 1996 paper expounding a “particularist” account of the picture theory, draws a good deal on these notes, before hooking his discussion up with the *Tractatus* itself. However, we find no trace of this thought in the notebooks from the period 1914-1916, which contain the thoughts he recorded between the time of writing of those earlier notes and the time of final composition of the *Tractatus*. On the contrary, the notebooks open in August/September 1914 with frequent reference to “subject-predicate propositions”; the passage

quoted earlier from October 1914 also contemplates a “Russellian” form; and later, in June 1915, we get the totally explicit remark, “Relations and properties, etc. are *objects* too” (1914-1916: 61 (16.6.15)).

And after the publication of the *Tractatus*—when Wittgenstein is after all very well placed to explain what he had in mind in writing it!—he remarks explicitly (in a conversation recorded by Desmond Lee in 1930-31), “Objects [as the word was used in the *Tractatus*] also include relations; a proposition is not two things connected by a relation. Thing and relation are on the same level. The objects hang as it were in a chain” (Wittgenstein, 1930-1932: 120).

c) Excursus on kinds of evidence: “textual”, not “circumstantial”

Now, much of the evidence just canvassed goes a long way to showing what Wittgenstein was *thinking about* as he worked on what would become the *Tractatus*: namely, quite likely, about forms of fact individuated roughly along the lines Russell did (even if not at all *conceived* as Russell conceived of them). But our real interest is in the question (as we might put it) what he *should have* been thinking about. That’s a flippant way to put my point: but I mean something like, where does the train of thought that he started with—namely, quite probably, with Russell’s puzzling doctrines of “logical objects”, and in particular logical forms—properly end up? Which way of understanding the notion of an object, and the correlative notion of a configuration of objects, sits better with the other doctrines of the *Tractatus*? (Thus, for instance, the mere fact that he gives a “Russellian” example of an elementary form at §5.5541 doesn’t by itself tell for or against anything—especially in view of the fact that the examples from earlier in the book sounded “particularist.” What we must ask is: whether anything of importance hangs on the examples’ being one way or another.) I don’t want to say that questions of textual interpretation are entirely independent of the kind of “intellectual biography” I’ve been discussing—or, more

generally, of questions of the historical context out of which the ideas under examination arise.

But I do want to say that the latter can't exhaust the former: can't, in this case, straightforwardly provide an answer to our interpretative question. This sort of evidence is suggestive, but it can't be definitive.

To illustrate my point here, it helps to observe that Anscombe and Ricketts both *notice* this textual evidence but take themselves to be justified in dismissing it: thus Anscombe, in response to Wittgenstein's *Notebooks* remark that "Relations and properties, etc. are *objects* too," postulates that Wittgenstein must have changed his mind between the *Notebooks* and the *Tractatus* (though she doesn't register the similar remarks he makes about the *Tractatus* post-publication!), arguing, as we saw earlier, that "if Wittgenstein held that objects fell into such radically distinct categories as functions and individuals, it is an incredible omission not to have made this clear" (1959: 109n1). Meanwhile, Ricketts, in the face of the remark Wittgenstein made *after* the *Tractatus* was published, to the effect that Tractarian objects include relations, and "a proposition is not two things connected by a relation," simply records bafflement at the notion of a "relation" that would result, without relenting in his "particularist" reading. As Ricketts puts it:

This is a peculiar remark. If an atomic fact is not two objects connected by a relation, then there seems to be no ground for calling any constituent thing in it a relation. Whatever Wittgenstein may have had in mind here, this view of relations is utterly unlike either Russell's or Frege's. (1996: 98n32)

d) Against the "textual" arguments for the "particularist" reading

Let me start by considering the quick textual argument we just saw Ricketts give: the claim that, if relations are to be just another sort of Tractarian object, we can no longer recognise them as *relations*, in anything like Russell's sense. I think that the story I told earlier, drawing on the work of Pears and McGuinness for instance, about Wittgenstein's engagement with Russell's

thought equips us to dismiss this argument, by showing us what the point of calling a certain object a relation might be.

Consider the crude model of a world with simple objects {a, b, c, d, F, G, R, S}, and with existent atomic facts Fa, Fc, Ga, Gb, aRb, bRa, bRc, dRa, aSb, bSa, bSc, bSd, cSd, dSa, dSb, dSc and nonexistent atomic facts Fb, Fd, Gc, Gd, aRc, aRd, bRd, cRa, cRb, cRd, dRb, dRc, aSc, aSd, cSa, cSb. Now, the typographical distinctions among the names I've given the objects are inessential: they're just a visual aid. The point is to see that we can reconstruct notions of particular, property, and two-place relation from these patterns of combination. For example, R only appears in facts involving two other objects, and F only appears in facts involving one other object, while a appears in facts involving either one or two other objects. On this basis we can distinguish among the *forms* of these objects, in the sense of §2.0141: among the possibilities of their occurrence in atomic facts.²¹

Now, Ricketts would perhaps respond that, nevertheless, what is missing is an *account* of these systematic differences, by appeal to, say, what Frege would call R's "unsaturatedness", or as Russell might put it, its capacity "actually to *relate* objects." It is presumably in this sense that, as Ricketts wrote, "there seems to be no ground for calling any constituent thing in [one of these facts] a relation.... [T]his view of relations is utterly unlike either Russell's or Frege's." But of course a "Russellian" reader of the *Tractatus*, far from hearing it as a criticism, will happily embrace this latter implication. It is Wittgenstein's "*Grundgedanke*", his fundamental thought, that the logical constants do not represent: or, otherwise put, that logic is not the science of a particular class of objects at all. And it is on this sort of ground that he rejects both Frege's

²¹ Stenius (1960: 69ff.) presents a similar model to illustrate the notion of "forms of object", though he takes what we have been calling the "Russellian" reading to be unequivocally correct.

account of the functional structure of atomic propositions and Russell's inclination to explain the theory of types in terms of ontological categories rather than simply laying it down as a set of symbolic rules (cf. §§3.33f.). Ricketts's correct observation that the treatment of relations implicit in the "Russellian" understanding of Tractarian objects is very unFregean, and indeed unRussellian, is in this sense no objection to the reading.

Let me move on to the solidly "textual" argument with which we began our discussion of the "particularist" reading: Sellars's conviction that §3.1432 is unequivocal:

Not: "The complex sign 'aRb' says that a stands in the relation R to b," but rather: *that* 'a' stands in a certain relation to 'b' says *that* aRb.

Now, the first thing to note here, as in fact we did in our discussion of the difference between Anscombe's "particularism" and Sellars's, is that there is nothing in the text to ground the supposition that 'aRb' is an elementary proposition. In fact, §3.1432 appears in a context which is palpably a discussion of propositions in *general*; indeed the possibility of a "completely analysed" proposition has not yet been broached.

The second thing to note is that the central point of §3.1432 is the *distinction between fact and object*. The point is that the picture, like "what's pictured", is a fact; and that it pictures *by being* a fact of the same form as the possible fact it pictures. But this *general* point can be made without any commitment as to which elements of the picture stand for objects and which indicate how the objects are with respect to one another.

Now put these two points together. A theme running through the *Tractatus* is the idea that:

From [colloquial language] it is humanly impossible to gather immediately the logic of language. Language disguises the thought: so that from the external form of the clothes one cannot infer the form of the thought they clothe.... (§4.002)

If what is at issue in §3.1432 is an ordinary-language proposition of the (superficial) form ‘aRb’ (and again, there is nothing in the context to suggest that ‘aRb’ is a *fully analysed* form), then we cannot even be sure that ‘a’ and ‘b’ name Tractarian objects, let alone that ‘R’ doesn’t (or doesn’t *only* do this—recall the way Anscombe thinks of ‘ ξ is red’). Again, without even knowing what constituents will show up in the full analysis of a given proposition, I can make the *general* point that displaying expressions in combination (§3.14) constitutes displaying a *fact* (§3.14), and in virtue of that is capable of expressing some *other* fact. (This rests ultimately on simple names’ going proxy for simple objects, and the proposition’s portraying those objects as being a certain way by itself being a certain arrangement of those names—but only *ultimately*, because one cannot tell from the unanalysed proposition what the simple names in question are.) But in order to illustrate the point with an example, one must still treat some aspect of one’s notation as that which (ultimately) shows how the objects hang together. This is the rôle that ‘R’ is playing at §3.1432.)

Anscombe herself sees something like this—at least, she sees that ‘aRb’ as it occurs in §3.1432 needn’t be understood to be fully analysed—but she nevertheless argues, as we saw, that “if Wittgenstein held that objects fell into such radically distinct categories as functions and individuals, it is an incredible omission not to have made this clear” (*Introduction*, 109n1). But our discussion of Ricketts’s similar rejection of this idea shows also why a “Russellian” can simply take Anscombe to be misguided here. The distinctness of particulars and universals is of radical importance for Frege and for Russell (in different ways), but—so the “Russellian” may hold—not for the author of the *Tractatus*.

e) Agnosticism about objects and configurations

In short, the case the “particularists” make for their reading(s) of the *Tractatus* isn’t compelling. But the fact that the book even appears to admit of this reading is significant. As we have already noted, one of the steps that Wittgenstein takes to be necessary for a proper understanding of the nature of the proposition, of truth and falsity, and of the relation of logic to propositions, is the refusal to give an account of the elements of the apparatus of logic—the truth-functional operators, the quantifiers, even Russell’s “logical forms”—as though they had (even peculiarly “general”) *content*. I have been arguing that an analogue of this point applies to the logical categories in terms of which one gives an account of elementary propositional articulation as well. Frege, like Russell, takes himself to be obliged to provide a specific account of categorial structure as a framework for his logical apparatus; Wittgenstein holds that this is more than necessary (and hence impossible). But by abstracting from the details of the nature of propositional articulation, and referring only to the mere *fact* of such articulation, Wittgenstein lays the ground for the possibility that such articulation may not be as either Frege or Russell envisions it. Even if, as a matter of biography, Wittgenstein happened to have in mind, during most (but not all) of the period during which he was composing the *Tractatus*, a categorial analysis along the lines of Russell’s or Frege’s (and even if traces of this fact of biography remain in some of the wording of the text), the train of thought he pursued ends up at a point where, for all *logic* has to say about it, the structure of elementary propositions might be quite other than that.

4. Functions in the *Tractatus*

We have seen, first by reflection on the contrast between Frege’s treatment of quantification and Wittgenstein’s, then by considering how the remarks in the early pages of the *Tractatus* on

“forms of object” and the picture theory emerged from an engagement with Russell’s puzzles over “logical forms,” that an important element in the Tractarian account of logic—indeed, one aspect of Wittgenstein’s “*Grundgedanke*”—is its refusal to give a specific account of the categories of “object” in which elementary propositions are articulated. Now, this is significant because it represents, precisely, a divergence from “Fregeanism” as I characterised it in Chapter II. As we saw then (*e.g.* in our discussion of the work of Ricketts and Dummett), Frege’s functional distinction between object and concept (and its generalisation up the hierarchy) is of a piece with his account of quantification. But Wittgenstein, in response to the difficulties Russell faced in giving an account of propositions in terms of their categorial articulation, means explicitly to refrain from getting into the details of this categorial articulation. Logic begins with the capacity for propositions to be true or false²², which is explained in terms of *picturing*; the account of this in turn is explicitly given in terms of articulation, but that is to say, in terms of the *very idea* of articulation, not of a particular account of it. And as we saw in the first section of the present chapter, Wittgenstein’s account of generality, too, does indeed rely on the notion of *articulation* at the level of the elementary proposition; but it refrains, again, from giving an *account* of that articulation (except to “locate” it, as it were, in the objects, in contrast with Russell’s attempt to make “logical forms” further relata involved in propositional attitudes).

In short, while both Frege’s remarks about objects and functions and the notion of the form of an object deployed in the opening pages of the *Tractatus* speak to the question of the articulation of elementary propositions, indeed in both cases on the basis of considerations deriving from reflection on inference, the significant difference (from the point of view of the present enquiry) is that Frege takes the content of the categorial analysis, even at the level of elementary

²² I take it that this is the import of §5.5521, for instance: “... if there were a logic, even if there were no world, how then could there be a logic, since there is a world?”

propositions, to be *settled* by that reflection on inference, while the doctrine of the *Tractatus* is precisely the reverse of this. And it is in this sense that I reject Rödl's claim that the *Tractatus* is a more thorough expression of Fregeanism about the structure of thought than Frege's own work. We saw in Chapter II that the controlling thought of Frege's account of propositional articulation was precisely that it could provide the framework for a systematisation of inference—if not, indeed, that it would be *yielded* by such a systematisation. But what we have just seen about the *Tractatus* is that its conception, sketched in the opening pages, of the unity of the elements of an elementary thought does not fit that description.

However, there is one serious objection I must consider to this contention of mine that the Tractarian conception of elementary propositional structure is unFregean. For Wittgenstein writes, “I conceive the proposition—like Frege and Russell—as a function of the expressions contained in it” (§3.318); and later and more generally, “Where there is composition, there is argument and function” (§5.47). It thus sounds as though, even if Wittgenstein does not conceive of it as logic's task to specify the particular categories into which simple objects fall, still those categories must be such as to be susceptible of an analysis in functional terms. If indeed he is not thinking of the very same categories as Frege—the only difference being that, where Frege took the explicit specification of the categories to be a necessary part of logical theory, Wittgenstein is claiming that it is unnecessary and hence inappropriate—then, at any rate, even so, he is not thinking of them as unamenable to the sort of function-argument analysis I claimed in Chapter II was characteristic of the Fregean conception of propositional articulation.

The point perhaps appears more acute when we see, in the sections following §5.47, that the appeal to function-argument analysis bears even more similarities to Frege's than so far recorded. Thus at §5.523 we are told that “The generality symbol occurs as an argument”: and this point

appears to be akin to Frege's, that the attribution of function-argument structure to a content of possible judgment becomes univocal when that content involves generality.²³ Now, of course, this is not to say that Wittgenstein's conception of the functional articulation of elementary propositions is Fregean in every detail; on the contrary, for instance, it is well-known that Wittgenstein utterly rejects Frege's conception of propositions as having truth values as *Bedeutungen*, and so the particular account Wittgenstein would give of the functional composition of an elementary proposition (if he thought it proper to give one at all) would be different from Frege's. Instead, as §3.318 implies, in understanding a *Satz* as composed of function and argument, it is the *proposition itself*, not a truth value, that is the value of the function in question. This is of a piece with Hylton's observation (mentioned in note 18 above) that frequently for Wittgenstein in the *Tractatus*, as for Russell in *Principia mathematica*, 'function' *tout court* means *propositional* function. However, in explicating "Fregeanism" in Chapter II, I explicitly included as exemplars approaches that refrained from understanding predicates, with Frege, as functions *into the set of truth values*. What I argued there was of a piece with Fregeanism about the relation between propositional articulation and the systematisation of inference was the bare idea that the former was to be understood in function-analytical terms. And this, §§3.318, 5.47 and 5.523 suggest, appears to be present in the *Tractatus* as well.

(Another way of putting this objection would be to say that, contrary to the thrust of this chapter so far, the fact that the author of the *Tractatus* neglects to give a specific account of the categorial structure of elementary propositions is after all inessential: for the various remarks just

²³ "For us the different ways in which the same conceptual content can be taken as a function of this or that argument has no importance so long as function and argument are fully determined. But if the argument becomes *indeterminate*... then the distinction between function and argument acquires significance with regard to *content*" (*Begriffsschrift*, §9; emphasis in original).

quoted reveal that he takes it to be functional—if not in exactly Frege’s sense, then anyway in a sense sufficient to brand his view a version of “Fregeanism” as we used that term in Chapter II.)

A quick response to this objection might run as follows: We have already seen that Wittgenstein’s account of generality, though it makes reference to the articulation of elementary propositions, makes no reference to the particular way(s) in which they are articulated. Whereas (again recalling Ricketts’s and Dummett’s expositions) the grounds for Frege’s distinction between singular terms and predicates appear to lie in his treatment of inferences involving generality—or, if that is too much to say, it is at least the case that the two notions are profoundly linked—Wittgenstein treats all elements of atomic facts as “objects”, likewise all elements of elementary pictures as “names”, and any expression characterising the sense of an elementary proposition as a potential basis upon which to launch a collection of propositions over which to generalise, by replacing that expression with a (propositional) variable. If Wittgenstein persists in using function-argument vocabulary to speak of the elements of elementary propositions, goes the quick response to the objection, this is an inessential relic from his having inherited his problems from Frege and Russell. If the claim that elementary propositional structure is functional in character does no work, then (by the *Tractatus*’s own principles) it should be abandoned.

Now, it would be more satisfying if we could somehow *demonstrate* that the function-argument vocabulary of the *Tractatus* is inert. (The quick response just sketched would, after all, otherwise be rather uncharitable: for §§3.318, 5.47 and 5.523 (as well as other passages involving the same vocabulary) are awfully unambiguous.) And to this end, we can put a finer point on our question: Can we have, on the Tractarian conception of propositional articulation, two propositions consisting of the very same elements, where what differentiates between them

is *only* the *mode of combination*? (Compare our parallel enquiry in Chapter II *vis-à-vis* Frege.)

If not, then there can be no harm in Wittgenstein's references to the articulation of elementary propositions as functional.

The assertion that the elementary proposition "is a connexion, a concatenation, of names" (§4.22) may suggest that the answer to this question is no: the mode of combination alone will never distinguish two elementary propositions consisting of the same elements. (I am setting aside the case of a difference in *order*, which we may suppose can serve to indicate the difference between propositions—of the same logical form, of course—involving asymmetrical relations relating objects of the same form: e.g. the difference between 'a is darker than b' and 'b is darker than a'.) The word 'concatenation', after all, suggests that there is just one mode of combination of names. And recalling our discussion of Wittgenstein's engagement with Russell's puzzles about "logical forms", it may seem as though this is the shape the Tractarian account *must* take. For if, even having settled the question what objects (with what forms) constitute a certain proposition, we have not yet identified the proposition, on the grounds that different propositions with different forms can consist of the very same objects, it is hard to see how we have any advance over Russell: it looks as though, to characterise the proposition completely, we must describe, over and above its objects, its form.

However, first of all, we oughtn't get too hung up on 'concatenation'. It (more properly, the word in German it renders, '*Verkettung*') appears in the book only once. The proposition commenting directly on §4.22 adds: elementary propositions "consist of names in immediate combination" (§4.221). To say that the combination of names is immediate ("*unmittelbar*"), not mediated, is to say that no further *element* provides the link between name and name. But all the other discussions of the structure of atomic facts, elementary pictures and elementary

propositions fulfil this requirement while having no implication that there need be only one mode of combination for a given (ordered) collection of elements:

An atomic fact is a connection of objects (entities, things)....
The configuration of the objects forms the atomic fact.
In the atomic fact objects hang one in another, like the links of a chain.
In the atomic fact the objects are combined in a definite way.
The way in which objects hang together in the atomic fact is the structure of the atomic fact....
The picture consists in the fact that its elements are combined with one another in a definite way....
That the elements of the picture are combined with one another in a definite way, represents that the things are so combined with one another....
The propositional sign consists in the fact that its elements, the words, are combined in it in a definite way....
The essential nature of the propositional sign becomes very clear when we imagine it made up of spatial objects (such as tables, chairs, books) instead of written signs.
The mutual spatial position of these things then expresses the sense of the proposition. (§§2.01, 2.0272ff., 2.14, 2.15, 3.14, 3.1431)

None of these ways of expressing the idea of atomic or elementary articulation implies that there can be only one “definite way” for a given set of elements to be combined; indeed many of them positively suggest that there can be many. This is entirely compatible with the essence of Wittgenstein’s resolution of Russell’s puzzles about “logical forms”, which is to dispense with the idea that they are further *constituents* of propositions needing their own substantive account. There are objects, which have it in them to hang together with one another in various ways (and again, the point is that the ranges of such possibilities “lie in the nature of the objects”, and do not need to be accounted for separately from them); there are facts, which consist of objects’ doing so; there are pictures of such facts, in which pictorial elements go proxy for the objects and hang together with one another in the same ways in which the objects can do so; and in particular (an instance of this last) there are elementary propositions, in which the pictorial elements are names. And this is enough, according to the *Tractatus*, to secure propositions’ position as truth-bearers, which is in turn enough to bring with it the whole of logic.

If this is right, then the abstraction from the details of categorial structure with which, since the beginning of this chapter, we have been dealing, can be understood to extend even as far as

abstracting from the question whether elements can be combined in more than one way—and that is, whether such combination is susceptible of analysis in functional terms. That Wittgenstein (once) chooses the word ‘concatenation’ is meant to remind us not to look for a further element mediating the combination of names in an elementary proposition (and objects in an atomic fact), not to insist that there can be only one mode of combination. And that Wittgenstein (rather more than once) speaks about elementary propositional structure in terms of function and argument can be interpreted not as an essential feature of his account (for he does not make use of it²⁴) but as a relic of the way in which his interlocutors had framed the discussion.

5. Formalisability and the route to Wittgenstein’s later work

But if the details of the categorial structure of elementary propositions (though not the very *fact* of that structure)—even the question whether that structure is functional in character—are beyond the scope of logical theory, what becomes of the idea of the *formalisability* of formal inference? For recall our argument from Chapter II: we maintained there that formalisability presupposes amenability to some form of function-argument analysis.

Essential to the *Tractatus*’s conception of inference is the dictum, “[t]hat the truth of one proposition follows from the truth of other propositions, we perceive from the structure of the propositions” (§5.13). Together with the idea that “[w]e can bring out these internal relations [*sc.*

²⁴ Does he not make use of it even in his account of expressions? Again, §3.318: “I conceive the proposition—like Frege and Russell—as a function of the expressions contained in it.” But the account of expressions itself, immediately preceding this proposition, makes no essential use of the notion of a function. True, “[a]n expression is... presented by a variable, whose values are the propositions which contain the expression” (§3.313)—but Wittgenstein explicitly rejects the approach of giving an account of such a propositional variable as a function from some class of arguments into the set of propositions. What is essential to the idea of a propositional variable is *simply* that it has a list of values. “What values the propositional variable can assume is determined. The determination of the values *is* the variable. The determination of the values of the propositional variable is done by *indicating the propositions* whose common mark the variable is.... The determination will therefore deal only with symbols not with their meaning.... The way in which we describe the propositions is not essential” (§§3.316f.).

those in which the structures of propositions stand to one another] in our manner of expression, by presenting a proposition as the result of an operation which produces it from other propositions” (§5.21), this sounds very much like an expression of the idea that a notational system can be devised so as to render all such inferential relations patent. And this in turn sounds like a version of the Fregeanism we described in Chapter II. Indeed, after discussing the fact that, in ordinary language, we often use the same sign with different meanings (indeed, as his examples suggest, with meanings of different logical categories, though he does not say this explicitly), he continues:

In order to avoid these errors, we must employ a sign language [*Zeichensprache*] which excludes them, by not applying the same sign in different symbols and by not applying signs in the same way which signify in different ways. A sign language, that is to say, which obeys the rules of *logical* grammar—of logical syntax. (§3.325)

And if this very “sign language” is to be the one in which we bring out the internal relations among propositions so as to reveal their inferential connections—and there is no indication in the work that it is not—then it looks very hard indeed to maintain that his conception of logical structure is meant to be any less of a piece with the systematisation of inference than the conception discussed in Chapter II.

This may well seem unsurprising if (as suggested by our discussion of the “biographical” question what categorial analysis he had in mind when writing the *Tractatus*, even if the official *Tractatus* doctrine is to abstract from its content) Wittgenstein is taking Frege’s and Russell’s notational systems as for the most part acceptable, and jibbing primarily at the *explanations* they give for their systems. Passages such as the following (picking up where the previous quotation left off) reinforce this impression:

The logical symbolism [*Begriffsschrift*] of Frege and Russell is such a language [*sc.* one which obeys the rules of logical grammar], which, however, does still not exclude all errors. (§3.325)

(It “does still not exclude all errors” because, for instance, Wittgenstein appears to advocate for the elimination of the identity sign, on the grounds that, “Roughly speaking: to say of *two* things that they are identical is nonsense, and to say of *one* thing that it is identical with itself is to say nothing” (§5.5303). But even in this case, the conclusion of his discussion is simply that ‘=’ “is therefore not an essential constituent of logical notation” (§5.533), not that everyone ought to stop using it. And as for the case of the truth-functional connectives, Wittgenstein nowhere insists that they would be absent from an ideal notational system; his claim is merely that they “do not represent”, that is, that the *explanations* Frege and Russell give with their introduction of the logical constants are illegitimate. Analogously, Hylton has suggested, plausibly, that Wittgenstein does not reject the import of Russell’s theory of types, but only his explanation of it.)

But it is worth keeping in mind that the goal of the *Tractatus* is not, after all, to *devise a begriffsschrift*, as it was quite clearly Frege’s goal to do, for instance. Wittgenstein’s goal for the work is to enquire into the nature of logic, motivated by dissatisfaction with some of Frege’s and Russell’s remarks—made, as it happens, in the course of devising begriffsschriften. In this sense, Wittgenstein was not setting out to revise the notational systems of Frege and Russell, much less to argue against the very possibility of such a system—but nor was he closed to the prospect that his enquiries might result in the requirement of some revision (as the case of identity suggests). Thus, even if we shouldn’t be surprised that he begins by taking for granted, in some sense, the availability of an adequate formalisation of inference, indeed one which at once yields an exhaustive account of propositional articulation, we can still ask whether the philosophy of logic to which he is driven doesn’t risk undermining it.

And again, we have argued that reflection on logic—on generality, and on the way in which the nature of propositional unity secures the aptitude of propositions for truth and falsity, and so the applicability to them of logic itself—leads Wittgenstein to generalise the idea of elementary propositional structure. We have argued, indeed, that this idea is generalised to a point where (even if Wittgenstein does not notice this) even the question whether it can be construed in *functional* terms (in such a way that it is even *available* for the kind of integration into a broader account of inference that Frege’s approach constitutes) is left open.

And indeed, the examples Wittgenstein gives in the opening pages of the *Tractatus* of this now broadened notion of elementary propositional structure—or rather, the examples he gives of the forms of object occurring in atomic facts, whose forms are shared with the names going proxy for them in elementary propositions—betray some tension with the account of inference to follow. Wittgenstein tells us that “[s]pace, time and colour (colouredness) are forms of objects” (§2.0251).²⁵ We might have expected this to hook up with the account of inference to follow:

If the truth of one proposition follows from the truth of others, this expresses itself in relations in which the forms of these propositions stand to one another... these relations are internal, and exist as soon as, and by the very fact that, the propositions exist. (§5.131)

That is, since the notion of the form of an elementary proposition of course moves in tandem with the notion of the forms of the names occurring in it, the examples of forms of object cited above might lead us to suppose that such forms are relevant not just to inference involving generality but also to immediate inferential relations among elementary propositions.²⁶ But it is

²⁵ I don’t mean to suggest that Wittgenstein is absolutely committed to this list; indeed the result of our discussion above of the “particularist” and the “Russellian” readings suggests that he can’t be (though he certainly does state it categorically enough, at least at this stage in the dialectic). But the examples are instructive, regardless whether we are eventually intended to throw them away.

²⁶ After all, space, time and colour have *structure*, and indeed *internal* structure, in the sense used at §5.131 (and explicated at §§4.122ff.). “We [could not] present spatially an atomic fact... which contradicted the laws of geometry” (§3.0321)—but if “the laws of geometry” are included in the form of spatial objects—and it is hard to see

well known that the *Tractatus* denies that any such relations obtain: “[f]rom an elementary proposition no other can be inferred” (§5.134). More generally, the talk of particular forms seems to *disappear* when we get to the account of inference in terms of truth operations offered in the §§4s and 5s. Again, the very fact of elementary propositional structure is relevant to inference, in the sense that we can form general propositions by deploying truth operations on a set of elementary propositions collected in virtue of some structural property they have in common—some shared expression, for instance; and of course general propositions will, in general, stand in inferential relations with the elementary propositions that constitute the bases of their formation, as well as with other results of truth operations on those elementary propositions. But there is no room in this account of inference for *systematic differences* in the rôles names of different forms play in inference. Indeed, this much follows immediately from what we saw in the §§5.55s about how it is not for logic to canvass the forms of elementary proposition.

But what these reflections highlight is an incipient tension within the *Tractatus*. On the one hand, it is true that the details of the account of inference are given exclusively in terms of the general theory of truth operations, and indeed the term ‘logical’ tends to be reserved for this sort of context.²⁷ But on the other hand, the examples Wittgenstein feels inclined to give of objects’ forms in the early pages bring out the sense in which elements of the *Tractatus* picture contain the resources for recognising the relevance of particular forms for inference (that is, not merely for inference involving generality, drawing on the abstract notion of elementary propositional structure but not on its details; but also for inference whose validity depends precisely on those particular details). It is a further commitment to the idea that truth operations exhaust inference

what the point of such a notion of form would be otherwise—then “we could not present” such a (putative) atomic fact in *any* way.

²⁷ But there are exceptions: e.g., in §§2.012 and 2.0233, before a word has been breathed about the truth functions and such, the word ‘logic’ is quite explicitly associated with forms of objects.

which leads Wittgenstein, in conflict with those elements of the picture, to adopt the axiom of independence of elementary propositions, and hence for instance to claim that what at first appears to be (and indeed, as we saw, was given explicitly as an example of) a particular form of objects, namely colour, must in fact resolve into truth-functional relations: at §6.3751.

Now, by the words “further commitment” I do not mean to suggest that the commitment to the truth-operationality of inference is superficial or gratuitous. On the contrary, its motivation runs quite deep. Briefly, I think it runs something like as follows: We have seen that Wittgenstein’s “*Grundgedanke*” is that the “logical constants” (taken in a suitably broad sense) do not represent: that what is most unsatisfactory about both Frege’s and Russell’s approaches to explaining their notation is the implication that logical notions have *content*, and that logic is thus just the special science of this (admittedly very “general”) content. His response to this is, in broad strokes, what he refers to as “our fundamental principle [*Grundsatz*]”: “that every question which can be decided at all by logic can be decided off-hand” (§5.551). A bit more explicitly:

It is clear that everything which can be said *beforehand* about the form of all propositions at all can be said *on one occasion*....

One could say: the one logical constant is that which *all* propositions, according to their nature, have in common with one another. (§5.47)

His N-operation is designed to show how he can derive both truth-functional and (he believes) quantificational inference in this way, all at once, from the mere idea of truth- and falsity-aptness. His pre-*Tractatus* notebooks contain much agonising over identity in this connection: and the solution upon which he eventually hits, which we have already discussed, is to argue that identity isn’t essential to logic at all. But the thought that the particular forms of simple object, and so of elementary proposition, could also be relevant to inference would immediately and obviously undermine this approach. (For one thing, such forms are not something “which *all* propositions, according to their nature, have in common with one another.”) Indeed, the

“*Grundsatz*” quoted above is given, precisely, as a reason to hold that giving a catalogue of the elementary forms is not part of the province of logic.

Even so, as I say, the axiom of independence for elementary propositions stands in some tension with the picture theory, as is illustrated by his seemingly contradictory approach to colour discussed above. It is telling that his attempt at the resolution of colour ascriptions into more elementary, logically independent propositions fails—and more telling still that, once Wittgenstein recognises this tension in his work, he rejects, precisely, the axiom of independence, and embraces the relevance of particular forms for inference. This is perfectly explicit in “Some remarks on logical form,” a paper written in 1929. There he realises that (contrary to the doctrine of the independence of elementary propositions) there are propositions which can’t be construed as truth-operational combinations of simpler propositions, but which stand in logical relations one to another: in particular, propositions involving matters of degree—and he gives as his example colour ascriptions to places in the visual field. The conception of inference as a matter of the structure of propositions is there retained; the claim that there are “*ever so many different* logical forms”²⁸ is emphasised—even the truth-table method is retained (albeit in a modified form: for the non-independence of elementary propositions, Wittgenstein sees, requires us to discount those rows of a truth table which don’t represent “actual possibilities”²⁹).

This post-*Tractatus* rejection of the independence axiom is momentous, as is to be expected in the light of the sketch of its motivation I gave above. In that same paper Wittgenstein writes:

... we can only arrive at a correct analysis [of a proposition into its elementary constituents] by, what might be called, the logical investigation of the phenomena themselves, *i.e.*, in a certain

²⁸ (1929:31, Wittgenstein’s italics). The context makes clear that what is meant is forms of *elementary* proposition.

²⁹ We shall examine this point more closely below.

sense *a posteriori*, and not by conjecturing about *a priori* possibilities.... An atomic form cannot be foreseen. (1929: 30)

This echoes language from the *Tractatus*: the §§5.55s, as we have already seen, answer in the negative the question whether elementary propositional forms can be given *a priori*, adding the remark, “Only that which we ourselves construct can we foresee” (§5.556). So the claim, new to the 1929 paper, that, nevertheless, this analysis of elementary propositions is necessary for logic is in effect a concession that the “*Grundsatz*” of that book was misguided: logical questions cannot all be decided off-hand, and their answers do not all fall out, at once, of the very nature of the proposition. We may need to reflect on (*sc.* our thought about) the subject matter in question in order to arrive at its form—but this reflection is a *logical*, not an empirical, investigation.

This last claim is important. Wittgenstein is by no means intending to abandon the study of *logic* with this turn to the “in a certain sense *a posteriori*” investigation of the phenomena. On the contrary, the importance of the fact, if not the content, of elementary propositional structure has been present from the beginning of Wittgenstein’s enquiries; and the shift of attention now under consideration to the content of that structure was also driven by considerations of inference.

What I argued earlier in this chapter was that, unlike Frege, whose conception of (even elementary) propositional structure equated it with the structure assigned to propositions by a systematisation of inference, the *Tractatus* holds that elementary propositional structure is not a matter relevant to logical enquiry. Whereas Frege (and, in his own way, Russell) finds among the logical categories into which elementary propositional elements fall only those austere enough to be (allegedly) common to all judgments, whatever their subject matter—(Fregean) object, first-level function of one or two places, etc.—the author of the *Tractatus* (even if perhaps tacitly supposing that categorial analysis to be correct) makes room for the possibility of

a much wider, and richer, range of categories. And the author of “Some remarks on logical form” embraces this possibility.

Now, “Some remarks on logical form” may appear “Fregean” in its attitude toward logical categories after all—for it suggests that the systematic treatment of inference in the form of the truth-tabular method can accommodate this enrichment of the logical categories, by (as described briefly above) crossing out the rows of a truth table whose putative truth-value assignments have been revealed, through our “logical investigation of the phenomena,” not to be genuine possibilities. But quick reflection on this idea exposes its bankruptcy—or, at least, its distance from anything like the kind of *formal system* that is at the heart of the “Fregeanism” we discussed in Chapter II. When we recognise that the logic of truth functions is not sufficient to validate the argument attributed to De Morgan, “Every horse is an animal, so every horse’s head is the head of an animal”, we do not simply enrich the truth table for the two sentences by discounting the row where the premise is true and conclusion false. If we knew to do that, we would already know that the argument was valid, and would not need the truth-tabular method to begin with. On the contrary, we abandon the truth tables and deploy the finer-grained method of quantificational proofs. The case here is analogous: if, before we can “apply the truth-tabular method” to arguments involving logically dependent elementary propositions, we must know in what logical relations the propositions stand, then we are not obviously in need of the truth-tabular method; and even if we should choose to use it, the procedure as a whole—drawing up the table, enquiring into the “phenomena” and crossing out rows of the table accordingly, and

then checking the values in the remaining rows—does not itself amount to a mechanical method, even if the last step does.³⁰

Analogous considerations, by the way, hold of the “system” Sellars attempts to derive from the *Tractatus* itself. We saw earlier that Sellars reads the *Tractatus* not as refraining from settling the question how to understand elementary propositional forms but rather as, quite explicitly, placing everything predicative in the “form”, leaving only particulars as objects. Observing that Wittgenstein’s own displays of notation in the *Tractatus* tend not to respect this reading (but rather are in “PMese”, that is, the notation of *Principia mathematica*), Sellars describes a language, “Jumblese”, which would more perspicuously illustrate Tractarian doctrines about elementary propositions, in which (as, again, we discussed above) one expresses that two objects stand in a certain relation by placing their names in a certain relation *without using a further symbol* to “stand for” that relation, and one expresses that an object has a certain property by writing its name in a certain way, again without using a further symbol (but rather, perhaps, using a special typeface or colour). He recognises that this raises the following problem: how are we to understand second-order quantification, if elementary propositions perspicuously written do not contain predicate symbols?³¹ How will we indicate what a quantifier “over predicates” binds, if predication is represented not by separate symbols standing for predicates but by the manner in which we write the names in the proposition?

³⁰ One reason for which Wittgenstein repudiated this paper as “weak” so soon after he wrote it might be that he saw just the point made in the text: namely, that the recognition of the logical significance of elementary structure had much deeper implications for formalisability than the paper registered. (Another reason—closely enough related to the first that they could both be right—may be that he realised, a little belatedly, the deep implications the abandonment of the independence axiom would have for what we referred to above as the “*Grundsatz*” of the *Tractatus*.)

³¹ That this question has the force that it surely does is related to the point I made earlier, to the effect that, for the most part, Wittgenstein is taking for granted the essentials of the begriffsschriften of Frege and Russell, and enquiring primarily into the account to be given of them. A reading of the *Tractatus* according to which higher-order quantification is a flat-out impossibility would be, *ceteris paribus*, quite awkward—not least because Wittgenstein occasionally *refers* to second-order quantification without remark, as e.g. at §5.5261, but also because a divergence that fundamental from Frege’s and Russell’s logic would surely warrant some comment.

Sellars sketches an answer to this question by presenting the following “schema for translation from PMese into Jumblese”:³²

<i>PMese</i>	<i>Jumblese</i>
I. <i>Names of particulars</i> a, b, c, ...	The same letters written in a variety of neutral styles, the variety being a matter of height, the neutrality a matter of the use of the ordinary font: a, b, c, ...; a, b, c, ...; a, b, c, ...
II. <i>Statements</i> (not including relational statements, which will be discussed shortly) Green a, red a, ...	a , <i>a</i> , ...
III. <i>Statement functions</i> (1) <i>Predicate constant, individual variable</i> : Green x, red y, ...	x , <i>y</i> , ...
(2) <i>Predicate variable, individual constant</i> : fa, gb, ...	Names in neutral styles (see I): a, ...; a, ...; a, ...
(3) <i>Predicate variable, individual variable</i> : fx, gy, ...	Name variables in neutral styles: x, y, z, ...; x, y, z, ...; x, y, z, ...
IV. <i>Quantification</i> ($\exists x$) green x ($\exists f$) fa, ($\exists g$) ga, ... ($\exists f$) ($\exists x$) fx, ($\exists g$) ($\exists x$) gx, ...	($\exists x$) x ($\exists()$ a, ($\exists()$ a, ... ($\exists()$ ($\exists x$) x, ($\exists()$ ($\exists x$) x, ...
[V. <i>Relational statements</i>] Larger (ab), Redder (ab) R(ab), S(ab), T(ab), ... Larger (xy), Redder (xy), ... R(xy), S(xy), ... ($\exists x$) ($\exists y$) Larger (xy) ($\exists R$) R(ab), ($\exists S$) S(ab), ... ($\exists R$) ($\exists x$) ($\exists y$) R(xy)	^a _b , ^a _b ab, a b, a b, ... ^x _y , ^x _y , ... xy, x y, x y, ... ($\exists x$) ($\exists y$) ^x _y ($\exists \dots$) ab, ($\exists \dots$) a b, ... ($\exists \dots$) ($\exists x$) ($\exists y$) xy

But it is surely clear, though Sellars appears not to register the fact, that *this is not a formal system* (unless we are to constrain its expressive power *very* sharply, by setting limits on the

³² The following is, apart from a couple of inessential notational modifications, a quotation from Sellars (1962: 14-15, 16).

number of predicates and predicate variables it contains). For consider: we could perhaps agree, in the case of monadic predicate variables, to vary the height of the names we write by (say) one millimetre at a time: so that, in place of a standard system's infinite number of predicate variables ϕ_1 , ϕ_2 and so on, we would write names (or parentheses, in the case of predicate quantification) 1mm high, 2mm high and so on. This would of course be wildly impractical, but would not be at variance with the principle of a formal system. And we could do something analogous for the dyadic predicate variables: settling on a horizontal distance between names of, say, 1mm at a time to correspond with ψ_1 , ψ_2 and so on. However, consider the case of particular dyadic relational predicates: we must have an unending range of orientations in which to write two names, and we must be able to discriminate among them. The case of particular monadic predications is, if anything, even worse: we must have an unending supply of different fonts, *provided in advance* (since one hallmark of a formal system is that it is settled what counts as a formula of the system) and *discriminable one from another*. —In case it should appear that I am being unduly harsh on Sellars by quibbling with his decision to represent different predicates by writing names in different fonts, consider the point in this way. Suppose we chose, instead, to write an object's name in a given colour to say that the object was of that colour. We would have to have at our disposal the whole range of colours—and we would have to be able to discriminate between any two colours, *however close to one another they are*. In short, to exchange “PMese” for “Jumblese” is to replace the countable sets of predicates of each n-arity with “manners of writing names” which will have to have all the richness, all the “mathematical multiplicity” in Wittgenstein's phrase, of the properties they're meant to express—where this can involve (since after all predicates *do* involve) structures as rich as the real numbers (for instance). Whatever interest Sellars's system of notation holds, it does not show that the conception of

predication underlying “Jumblese” can provide a basis for a formalisation of higher-order quantification analogous to Frege’s or Russell’s.

I don’t know whether, presented with this objection, Sellars would have bitten the bullet and held that the conception of elementary structure in the *Tractatus* tacitly undermined the idea of formalisability (though the fact that he felt constrained to provide the account of second-order quantification he provided, such as it was, suggests that he would at least have felt uncomfortable about this). My claim is weaker than this: my claim is that, though Wittgenstein proceeds in the *Tractatus* as though formalisability along the lines of Frege’s (or Russell’s) begriffsschrift is unproblematic, he generalises the notion of elementary structure sufficiently to render the question of formalisability, as a matter of fact, open; and in his next work, he *does* embrace a conception of such structure that, indeed, is recalcitrant to formalisation in this sort of way (though he doesn’t register the fact in the paper itself).

And (as a *very* rough sketch of the next stage in the train of thought represented by Wittgenstein’s career), once this recalcitrance to formalisability sinks in, the “logical investigation of the phenomena themselves, *i.e.*, in a certain sense *a posteriori*, and not by conjecturing about *a priori* possibilities” takes centre stage, now with no presumption that the result of the investigation must be a systematisation of inference. Once the project of constructing a begriffsschrift is abandoned—or, at any rate, once this “logical investigation of the phenomena” ceases to be conceived as of a piece with the construction of a begriffsschrift—the very distinction between “elementary” and other propositions loses some of its point. But this doesn’t imply that the investigation of (to put it roughly) how the elements of our thought hang together must not be considered a logical enquiry, or an enquiry into the structure of discourse (or, perhaps better, structures, now that we have made room for the possibility that

different “phenomena” will evince different logical behaviour), rather than a psychological or anthropological one. As we discussed in the chapter on Wittgenstein’s later work, §92 of the *Philosophical investigations* acknowledges the appropriateness of the term ‘structure’ (of language) for the description of the object of those investigations, even while insisting that their context is quite different from the investigation of a complete, unitary inferential system.

And it is in this context that we can understand, finally, the sense in which Thompson, though he gives us no materials for the construction of a formal treatment of natural-historical judgments, can still be said to be engaged in an investigation of their logical form. In the coda that follows, I shall attempt to sketch how this connection can be spelled out.

Coda. The conception of logical form that emerges from our reflections.

I embarked on this project with a view to explaining the sense of ‘logical form’ at issue in (among other places) Thompson’s claim that what he calls natural-historical judgments have a distinct logical form, partly by hooking it up with (not only the later but also the earlier) work of Wittgenstein. So far (and we are almost at the end of the investigation), our results have been primarily negative. I began, in Chapter II, by describing, for the purposes of contrast, what I take to be, at least in abstraction from differences in detail, the mainstream understanding of the way in which the notion of logical form is applied to the compositional structure of a judgment, with its roots in Frege’s invention of the *begriffsschrift* (recognising all the while, as discussed in Chapter I, that ‘mainstream’ does not mean “universal”, since many philosophers of logic and language working today would consider that the presumption of unique reference contained in the definite description ‘the compositional structure of a judgment’ is false). Next, in Chapter III, I argued, following Thompson, that that mainstream understanding cannot give a satisfactory account of the structure of natural-historical judgments. In Chapter IV, I made a modest attempt to cast doubt on a proposal of Rödl’s to understand the sense of “logical form” at issue in Thompson’s work, as well as the idea of grammar as it is deployed in Wittgenstein’s *Philosophical investigations*, as having its home in the Kantian notion of transcendental logic. In Chapter V, I gave grounds for holding that the trajectory from Wittgenstein’s early work to his later constituted not so much a radical break with “Fregeanism” *after* the *Tractatus* as a

development of a nonFregean conception of logical articulation already present, if muted, in that work.

But if the sense of ‘logical form’ in which I am interested is not the Fregean one that emerges from the location of a judgment in an inferential system, nor the Kantian one that emerges from reflection on what is required for judgment to refer to intuition... then what is it?

My readers will be disappointed to learn that I do not have a full answer to this question. This is partly just the effect of my having had to spend as much time and space as I have on ground-clearing in the previous chapters. But it is not entirely explained by that. If my conjecture—for such is all it is at this point, since I have not yet carried out the thorough archæological examination of Wittgenstein’s writings from the period between “Some remarks on logical form” and the *Investigations* that would warrant any more confidence—is correct that the conception of grammar under investigation in the latter work is an evolution, with certain important modifications, of the idea expressed in the former of “the logical investigation of the phenomena themselves, *i.e.*, in a certain sense *a posteriori*, and not by conjecturing about *a priori* possibilities” (1929: 163); if, more generally, as I emphasised in Chapter IV, the sort of reflection on grammar carried out in the latter work does not proceed from, nor is justified by, any one *principle*—then I can certainly not give the *principle* of the conception of logical form in which I am interested, to hold it up for neat comparison with its “Fregean” and transcendental alternatives.

But there are some things I can say, both in specification of the conception and in justification of the idea that it is (still) a conception of *logical form*. First, we saw in the previous chapter how it grew out of Wittgenstein’s reflection on the idea of the forms of elementary proposition.

(Compare Thompson: “It is because in the end we have to do with a special form of judgment, a *distinct mode of joining subject and predicate* in thought or speech, that I am emboldened to say that the vital categories are logical categories” (1995: 267-8; emphasis added and removed).)

This was an issue with which Russell engaged explicitly, and indeed conceived of as a problem of *logic*: “[T]he way in which the constituents are combined in [a] complex... may be called the ‘form’ of the complex.... It is such pure ‘forms’ that occur in logic” (1913: 98). (The example with which Russell goes on immediately to illustrate these words is an atomic proposition.)

Frege, too, took his account of inference involving generality to yield an answer to the question of the nature of the elementary proposition (to the point, indeed, at which the very idea of an “elementary proposition” has little significance in his framework, since the type of combination involved there is precisely the same as in compound propositions: *viz.*, functional application¹).

And it is an old problem in philosophy—older, actually, than the project of the systematisation of inference.²

(It is worth observing that, at least *prima facie*, the question how the elements of thought hang together in a judgment looks to be orthogonal to the question which laws or norms govern thought just as such. We observed as an aside in the previous chapter that MacFarlane (2000) takes the idea that the norms of logic are, distinctively, constitutive of thought as such (“as opposed to a particular kind of concept use” (2000: 51)) to spell out one sense in which logic has been called “formal”, indeed the sense which he goes on to champion. But if there are a range of

¹ This is quite clear, funnily enough, in his “Compound thoughts”: where (as we saw in Chapter II) we are told that “it is natural to suppose that, for logic in general, combination into a whole always comes about by the saturation of something unsaturated,” of which the phenomenon of compound thoughts is then singled out as a “special case” (1923-26: 37).

² Consider Plato’s *Sophist* and Aristotle’s *De interpretatione* and *Categories*. Though it would be a spurious etymology to take our word ‘logic’ to have been derived from *hē epistēmē logikē*—in fact it comes from *hē logikē technē*—one can’t resist thinking that there would have been nothing unusual in Plato’s or Aristotle’s using the former phrase to denote those enquiries.

distinctive types of elementary combination, specific to distinct regions of discourse, then those forms of combination, it seems, will not be “formal” in this sense.³ I observed, though, the oddness involved in using the word ‘*formal*’ to express this idea; and it is clear that the idea of forms of elementary combination *does* after all line up perfectly nicely with the notion of a distinction between *form* and *content* or *matter*. If we are for this kind of reason suspicious of MacFarlane’s favoured account of the formality of logic, we can hold on to the idea of a life form as a *formal* idea, and the idea that the fact that a judgment is a natural-historical judgment is a *formal* (*logical*) matter, without affirming that “the norms constitutive of thought as such” make special reference to these notions.⁴)

Of course, the *Investigations* famously casts doubt on the idea of “a final analysis of our forms of language, and so a *single* completely resolved form of every expression” (§91; cf. §47); and with it, on the very idea of an elementary proposition. So if we mean the present discussion to give an account of a conception of logical form also at work in the *Investigations*, we must modulate it to take account of this change. But note, first, that our own account in the previous chapter of the trajectory from the *Tractatus* through “Some remarks on logical form” and to the *Investigations* makes good sense of the rejection of the notion of an elementary proposition. The suggestion of §47 is that “elementary” and “simple” make sense only in the context of a given analysis, for a given purpose; and we have seen that the result of thinking through the realisation expressed in “Some remarks on logical form” is that it is not in the context of some one such totalising analysis of language that we are to carry out an enquiry into the structure of propositions. (One

³ I say “it seems” in order to leave open the possibility that one may attempt, perhaps along Hegelian lines, to show that any account of “thought as such” must contain an account of the range of elementary forms after all. This is explicitly denied by *Tractatus* §§5.55ff., of course; whether it is more apt as a description of the view implicit in the method of the *Investigations* depends on whether there would be any point in calling the result of the piecemeal explorations of the structures of our thought “an account of thought as such.”

⁴ Of course, I cannot pretend to have engaged with the details of MacFarlane’s argument, much less to have defended an alternative, fully worked-out conception of the formality of logic.

goal of the reflections in the *Investigations* on meaning and understanding is to see how to avoid supposing that “if anyone utters a sentence and *means* or *understands* it he is operating a calculus according to definite rules” (§81), at the uncovering of whose structure our logical enquiry is aimed.) There is no reason to conclude from this that we must stop thinking about how our ideas hang together; on the contrary, it should be plain that, at that level of abstraction, that description applies quite well to what Wittgenstein is doing in the *Investigations*. (To put it in a slogan: this is after all surely what he means by ‘grammar’.)

It is true that Wittgenstein occasionally invites us to consider apparently structured utterances on the model of inarticulate exclamations: for instance, “‘Now I know how to go on!’ is an exclamation; it corresponds to an instinctive sound, a glad start” (§323). But he can hardly be taken to mean that articulation is inessential to language, even to these particular uses of language: for not only is that a preposterous view in any case, but his own discussions of the very same utterances elsewhere draw on their articulation, for example by putting them in the past tense (§660, and indeed in the same §323).⁵ The point must surely be to invite us, again, to *consider* these utterances in that new light, in order to shake ourselves loose of certain habits of thinking with respect to them—not to lay down once and for all that that is the only correct way to consider them. So the fact that, once in a while, Wittgenstein urges us not to think in terms of propositional articulation is an example of the way in which, on the approach of the *Investigations*, propositions are to be considered from a range of points of view, for a range of purposes; but it should not discourage us entirely from thinking in terms of “the ways in which our ideas hang together.”

⁵ I owe this general point, as well as the specific example of §§323 and 660, to John McDowell.

With the shift away from the idea of a complete analysis (and so also from the idea of elementary propositions), we get instead a focus on *regions of discourse*. This is clear at *Investigations* §90 (which comes smack in the middle of the discussion of the idea of a “final analysis” of language):

Our investigation is therefore a grammatical one. Such an investigation sheds light on our problem by clearing misunderstandings away. Misunderstandings concerning the use of words, caused, among other things, by certain analogies between the forms of expression in different regions of language.

And it is clear, more generally, from the way in which the *Investigations* proceeds: through “sketches of landscapes” traveled over “criss-cross in every direction” (Preface). Thus, for example, with respect to the “misunderstanding” alluded to above in the quotation from §81, about the connection between using language with meaning and using a calculus, Wittgenstein attempts to “clear it away” not by giving, once-off, a definitive analysis of “meaning” and a definitive analysis of “calculus” and showing just how they differ, but by describing a great range of our uses of a wide variety of related expressions: ‘meaning’, ‘understanding’, ‘proposition’, ‘name’; ‘mental process’; ‘game’; ‘rule’; and so on—bringing out the ways in which they hang together, and also the ways in which they come apart.

Though Thompson places at the centre of his enquiry the “natural-historical judgment”—which may make it seem as though his aim is the “final analysis” of some particular construction—he makes clear from the beginning that the fundamental interest the isolation of the natural-historical judgment holds for him is in the fact that it enables us to isolate the region of thought of which it is a part. Before even introducing the “natural-historical judgment”, Thompson is at pains to bring out the battery of concepts necessary for an appreciation of the phenomenon of life, a grasp of no one of which is possible independently of a grasp of the others. The irreducibility of vital concepts points up the autonomous status of discourse containing them; the

centrality of the “look to a wider context” required to establish even that the matter one finds before oneself constitutes a living organism, not to mention that it is eating (say), brings out this irreducibility, this autonomy of judgments made in one of these regions from those made in some other. (And the idea of the “look to the wider context,” which Thompson derives explicitly from Anscombe’s work, is just as clear in the *Investigations*, e.g. in the discussion of expectation.)

It is with this in mind that Thompson introduces the notion of a natural-historical judgment: that is, in the context of the search for definitions of the various “vital categories” which “together form a sort of solid block” (266). He tells us that the isolation of the natural-historical judgment will allow us to define ‘life form’ non-circularly as something which can be the subject of a natural-historical judgment; and once we’ve defined ‘life form’, we will have broken into the circle. His model here is Anscombe’s enquiry into the region of discourse that is talk of intentional action; her isolation of a certain sense of the question ‘Why?’ allows her to give an account of the sense of ‘reason’ at issue in discourse about intentional action, and thence of the whole battery of concepts essential to that region of discourse. In each case, Thompson suggests, the concepts at issue—*life form*, *reason for (intentional) action*—are *formal*, *logical* concepts because to bring something under one of them is not to say something *substantive* about it but simply to indicate in what region of discourse it has its home. We understand these concepts not by giving *Merkmale* for them but by grasping the respective forms of judgment in which instances of them can feature: a life form just is that which can be the subject of a natural-historical judgment; a reason for action just is that which can be given as an answer to the question ‘Why?’ in the sense Anscombe elucidates. “It is because in the end we have to do with a special *form of judgment*, a distinct mode of joining subject and predicate in thought or speech, that I am emboldened to say that the vital categories are logical categories” (Thompson 1995:

267-8; his emphasis). And we might as well recall that part of what in turn emboldens Thompson to hold that the natural-historical judgment is a special form of judgment is the negative result of his discussion of the ways in which one might try to reduce it to forms of judgment recognisable to standard quantification theory—a discussion which we have attempted to fill out in Chapter III of this dissertation, and a discussion which, in large part, turned (again negatively) on considerations of inference.

I alluded briefly above to the prominence of the notion of *grammar* in the *Investigations* (and the quotation from §90 is of course a canonical expression of its centrality to the method of that work). But this may seem, like the repudiation of the notion of an elementary proposition, to indicate a sharp break from Wittgenstein’s early work, including “Some remarks on logical form”—which paper, we have seen, spoke of “the logical investigation of the *phenomena themselves*, *i.e.*, in a certain sense *a posteriori*, and not by conjecturing about *a priori* possibilities” (1929: 163; emphasis added). After all, §90 begins as follows:

We feel as if we had to *penetrate* phenomena: our investigation, however, is directed not towards phenomena, but, as one might say, towards the ‘*possibilities*’ of phenomena. We remind ourselves, that is to say, about the kind of statement we make about phenomena.

And this may seem to cast some doubt on the emphasis I have been placing on the *continuity* of the trajectory from the *Tractatus*, through that paper, and into Wittgenstein’s later work. But, first of all, the “Some remarks on logical form” quotation isn’t as straightforward as it seems. It speaks of a *logical* investigation; and “*a posteriori*” is qualified with “in a certain sense.”

Admittedly, the previous sentence speaks of “*inspecting the phenomena* which we want to describe, thus trying to understand their logical multiplicity” (1929: 163; emphasis added). But when we are given some examples, their description includes the following:

One shade of colour cannot simultaneously have two different degrees of brightness or redness, a tone not two different strengths, etc. And the important point here is that these remarks *do not express an experience* but are in some sense tautologies. (1929: 167; emphasis added)

The 1929 paper gives none but the vaguest account of the requisite “logical investigation of the phenomena.” But I hope it is clear that the method of the *Investigations*—the reflective exploration of the structures of our thought, disconnected from the idea of a total systematisation of inference (which would bring with it a single notion of “elementary proposition”) as well as from the idea that the only structure to be investigated is the structure shared by all thought whatsoever, regardless of subject matter—can be understood as a more precise account of just the sort of investigation called for. Again, “Some remarks on logical form” shares with the *Investigations* the second of these “disconnects” from earlier conceptions of logic and logical form. It does not yet contain the first: on the contrary, the idea of a “total analysis” is front and centre. But I argued in the previous chapter that the shape the second “disconnect” takes is such as incipiently to undermine that idea; that is, to lay the ground for the first.

In any case, both Anscombe and Thompson have something analogous: that is, if we are after all to hear a contrast between the logical investigation *of the phenomena*, in a sense in which logic would be driven by something like “ontology,” and (on the other hand) the investigation of our grammar, the two of them are solidly in the latter camp. For Anscombe explicitly repudiates the approach to the enquiry into the nature of the intentional that would seek some “extra feature which exists when [an intentional action] is performed” (1957: §19); and Thompson, explicitly following her, “reject[s] what may be called a purely metaphysical approach” to his own subject matter and “takes refuge instead in the *representation*” of life (1995: 267).

It may also be supposed that, despite the opening sentences of §90 quoted above, Wittgenstein’s use of ‘grammar’ is also, indeed perhaps primarily, meant to remind us that his method is one of

reflection on our *use of language*, and not on a system of thoughts, “pure intermediaries” (§94) between language and the world. (In other words, when Wittgenstein tells us that “Our investigation is... a grammatical one,” the contrast is not only between language and *ontology*, as the two preceding paragraphs have presupposed (inspired by Wittgenstein’s own wording at §90), but also between language and *thought*.) But this, too, is of a piece with the move away from the idea of a single system of inference (and its concomitant notion of an elementary proposition) to the idea of the exploration of regions of discourse. (The discussion of “pure intermediaries” comes at the heels of that of the idea of a “final analysis” of language.) That is, there can hardly be a problem with recognising that one can sometimes just *think* what, at other times, one speaks or writes; the problem is with supposing that (of course “fully analysed”) thoughts constitute a single super-system, in virtue of its relation to which we can use our language as we do. And I have already discussed the shift in focus from single unitary system to regions of discourse, and I have given reasons for understanding Anscombe and Thompson’s work to be on board with this shift—despite (especially) Thompson’s liberal talk of thoughts and concepts, for example, in place of sentences and predicates.

Finally, let me address briefly one likely question a skeptical reader will be entertaining with respect to the conception of logical form I have been sketching in these pages: namely, *what good is it? What is it for?*⁶ (And this question may seem especially pressing in light of the fact that, in the *Investigations*—the work which (I am suggesting) constitutes a canonical expression of this conception—no explicit reference is actually made to a notion of form used in the way in which I use it!) Now, of course, the contrast implicit in the question is with “logical form” on the “Fregean” conception: where the very idea of form has its home in, and gets its point from,

⁶ Both Anil Gupta and Nuel Belnap pressed me on this question.

an account of *valid inference*. And in fact, our own conception has at least *negative* significance with respect to this very account: for if it is true to say that the sort of generality exhibited in a natural-historical judgment is, anyway, not of the sort to be treated by any of the approaches canvassed in Chapter III, then that is at least to say *something* (negative) about how such judgments figure in such an account.

But to expect me to be able to give an answer to the “What good is it?” question strictly parallel to that of the “Fregean” conception of form—something of the shape, “I use the notion of form to sort between Xs and Ys”—would perhaps be to presume too much in favour of that conception. As we have said (citing Plato and Aristotle as examples), the enquiry into the structure of discourse is an old enquiry indeed, and has been driven by its own concerns: notably, puzzles about meaning, the possibility of false judgment, and so on, which puzzles appear to be pressing whether or not they are posed in the context of the idea of a complete formal regimentation of the inferential behaviour of the sentences of a language (or of the thoughts they express). If it turned out that a single, fully general, one-off account of discursive structure, applicable indifferently to judgments from all regions of thought, were not available, this fact would not of itself dissolve those puzzles. On the contrary, the piecemeal, region-sensitive account of discursive structure we would be pressed to give instead would appear to inherit whatever claim the one-off account was originally conceived to have to be a matter of logic. And the same thought applies when we shift from giving such an account for judgments one at a time, to giving an account for whole batteries of fundamental concept, each group of which is essential and categorial for its region of discourse. At least, such has been the whole force of the argument of this dissertation.

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